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VOLUME X.

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Co

THE MEMORY OF
GIOVANNI POLLI, M.D.,
LATE OF MILAN,
TRUE SCHOLAR AND FRIEND,
OF WHOM
ILLUSTRIOUS MEDICAL ITALY
MAY WELL BE PROUD,
THIS TENTH VOLUME OF THE "ASCLEPIAD
IS
WITH SINCERE AFFECTION AND HOMAGE
DEDICATED.

2111:24

PREFACE

TO THE TENTH VOLUME OF THE "ASCLEPIAD."

WITH this, the fortieth number of the ASCLEPIAD, I bring to a close the tenth annual volume.

In next volume there will appear, from time to time, a new section consisting of historical excerpts from past masterpieces of physic, each selected as a classical reading rendering justice to first thinkers and discoverers, and relating the origins of some true advancements in the science and art of Medicine.

BENJAMIN WARD RICHARDSON.

25, MANCHESTER SQUARE, LONDON, W.,

Easter Day, 1894.

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THE TREATMENT OF DISEASE WITHOUT ALCOHOL.*

*A REVIEW OF MEDICAL PRACTICE IN THE WARDS OF
THE LONDON TEMPERANCE HOSPITAL.*

DURING the spring of last year—1892—the governors of the London Temperance Hospital did me the honour of inviting me to become physician to their institution and to take charge of twenty-five beds, or a larger number under pressure of applications for admission. I had been out of hospital practice since the year 1867, and had never expected to return to it; but the opportunity was so much to my taste, and the mode in which it came to me was so handsomely conceived, I could not help availing myself of it: I, therefore, in April last entered upon my duties.

I was left perfectly free to prescribe alcohol to the patients under my care if I thought such administration at any time necessary; the only stipulation being that if I did prescribe alcohol it was to be as a medicinal substance, with a special note in the register of the facts of the prescription, the doses of the remedy, and the dates in every case. I thought this most fair to me and to alcohol, and accepted the contract with all good faith and cheerfulness.

* Lecture delivered before the British Medical Temperance Association at the London Temperance Hospital on Friday, March 17th, 1893.

I state at once that from the commencement of the time during which I have had charge of patients I have not prescribed alcohol in any case.

In making this statement, that in the treatment of the cases under my care no alcohol has been administered, I relate a simple fact, without prejudice and without any shade of what is called fanaticism. I am no fanatic for or against alcohol. I entered on these duties with the clearest understanding that as a remedial agent I could use alcohol at will. I recognize also the action of alcohol. I know it to be a good paralyser, and that, acting as such on the minute circulation, it will open the flood-gates of the circulation when they are resistent, and will allow the nutritive blood to flow into the minutest point of its destination. I recognise alcohol, too, as a good antiseptic; and am not blind to the fact that, at much cost to the working powers of the body, it may be made to act as a febrifuge. I have not, therefore, disdained to acknowledge its therapeutical position; I have availed myself only of a splendid opportunity of ascertaining, on a large scale, what can be effected in the treatment of *formidable diseases affecting various classes of people without alcohol* as a common remedy. Suppose no such thing as alcohol existed, how could I, as a physician, treat the sick under my care? That was the question! It will be observed that I make use of and emphasise the words "formidable diseases affecting various classes of people." I do this for a special reason. It might be assumed that some particular classes of patients come to this institution for the aid it affords, total abstainers for example, who to some extent are favourable subjects for treatment without alcoholic stimulation; and it may candidly be admitted that if this were the fact

the results gained here would not be fair comparisons, because total abstainers are, in remarkable degree, the best patients for recovery from all recoverable diseases, quickly, safely, easily. But we have no speciality of any kind in this place, and it is often matter of observation that we have more professed non-abstainers than professed abstainers in our wards. It repeatedly happens that sick persons are brought to us because they are not abstainers. In short, no one is refused on the score of temperance or intemperance. We rule absolutely, Who enters here leaves alcohol behind, and there we stop. No difficulty follows the carrying out of this rule; the habit prevailing in the place communicates itself to all who are admitted with so dominant a force that I have never been solicited by a patient for any alcoholic drink, not even for the stomach's sake.

And, as we have no speciality for classes of persons, so we have no speciality in respect to the diseases that have to be treated here. We do not admit the directly contagious diseases, such as small-pox, scarlet fever, typhus, and the like, but we admit typhoid or, correctly speaking, *enteric fever* and all other forms of disease. As an illustration of the kinds of disease that have come under my own care, I give the following list of the first two hundred cases, occurring between April and October, 1892. I take them as they come from the register, which is most scrupulously kept by our able registrar, Dr. Wilde.

Diseases of the Respiratory System.

Pneumonia with emphysema, 2 cases; acute pneumonia, 7; lobar pneumonia, 4; broncho-pneumonia, 3; double pneumonia, 1; alcoholic pneumonia, 1; bron-

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chitis and emphysema, 3; capillary bronchitis, 1; acute bronchitis, 5; bronchitis with thrombosis, 1; bronchiectasis, 1; emphysema (uncomplicated), 2; hydrothorax, 1; hæmoptysis, 2; pulmónary congestion, 1; pulmonary thrombus, 1; pulmonary tuberculosis, 6; phthisis, 5; fibroid phthisis, 1; acute phthisis with meningitis, 1; pleurisy and emphysema, 1; diaphragmatic pleurisy, 2.

DISEASES OF THE CIRCULATORY ORGANS.

Rheumatic pericarditis, 1; rheumatic endocarditis with mitral lesion, 3; rheumatic endo-pericarditis, 2; rheumatic endocarditis with cerebral embolism, 1; tricuspid regurgitation, 1; aortic stenosis and mitral incompetency, 2; pericarditis, 1; endocarditis, 3; fatty degeneration of heart, 1; aneurism of the descending aorta and rupture, 1; aortic aneurism, 1; aortic stenosis, 2; mitral stenosis, 3; mitral regurgitation, 1.

Diseases of the Alimentary System.

Gastric ulcer, 3; acute gastritis, 2; infantile diarrhœa, 4; gastro-enteritis, 2; enteritis, 2; cirrhosis of liver, 3; dyspepsia, 5; hæmatemesis, 2; gastric hæmorrhage, 1; dysentery (chronic), 1; gastric catarrh, 1; tabes mesenterica, 2; diseases of pancreas and ascites, 1; abdominal lympho-sarcoma, 1; epithelioma of the sigmoid flexure and rectum, 1; cancer of large intestine, 1; cancer of rectum with perforation and peritonitis, 1; colonitis, 1.

Diseases of the Nervous System.

Cerebral hæmorrhage, 2; cerebral syphilis, 1; hysteria, 2; locomotor ataxy, 2; embolic hemiplegia, 1; specific myelitis, 1; disseminated sclerosis, 1; chorea, 3.

Diseases of the Urinary System.

Nephritis, 2; Bright's disease (chronic), 1; uræmia from Bright's disease, 1.

Diseases of the Uterine System.

Attempted abortion, 1; menorrhagia, 1; miscarriage, 2; retroversion of uterus, 1; puerperal septicæma, 1; carcinoma of uterus, 1; rupture of the perineum, 1; pelvic growth, 1; retroflexion of the uterus, 2; fibroid uterus, 1; uterine polypus, 1.

Rheumatic Diseases.

Simple rheumatism, 11; subacute and chronic, 6; acute, 4; rheumatic arthritis of occipito-atloid joint, 1; rheumatic arthritis, 2; rheumatic chorea, 1; rheumatic gout, 1; lumbago, 1.

Enteric Fever—5 cases.

Cases of Acute Poisoning.

By oxalic acid, 1; by corrosive sublimate, 1; by spirits of salt and impure metallic chloride, 1; by carbolic acid, 1.

Miscellaneous Cases.

Acute alcoholism, 3; anæmia, 4; dorsal caries, 1; obesity, 1; pertussis, 1; follicular tonsillitis, 1; simple pyrexia, 1; debility, 1; asthenia from privation, 1; lympho-sarcoma, 1; urticaria, 2; chlo osis, 1; syphilitic eruption (hereditary), 1; diphtheritic croup, 1; eczema, 1; pyæmic diphtheria, 1; psoriasis, 1; lympho-ataxis, 1

The list given above represents in the fairest manner and degree the practice that falls to my lot in the wards of this hospital. I average, as nearly as possible, twenty-five new cases per month. The two hundred cases given extend, therefore, to two-thirds of my first year of office, or a period of eight months.

LEADING RESULTS.

From these details I turn now to the leading results of practice as they are recorded in our register. We there divide the results into four great classes :—

1. Cases that have recovered and been discharged.
2. Cases that have been discharged as relieved.
3. Cases discharged *in statu quo*, or unrelieved.
4. Cases that have died, with notes of post-mortem appearances when a post-mortem was obtained.

Out of the two hundred cases that have been under treatment the returns run as follows :—

116 recovered	=	58 per cent.
44 were relieved	=	22 „
11 were discharged unrelieved	=	5.50 „
29 died	=	14.50* „
<u>200</u>		<u>100</u>

It may be worth while now to comment on some details of these results. It were too long a task in one lecture to touch on every case, although each one would no doubt form subject for useful lessons. I shall be content to touch on such of these, under each head, as bear most pointedly on treatment without alcohol.

RECOVERIES.

I may state in regard to the recoveries that in every instance the most scrupulous care was taken in diagnosis. If there were the slightest doubts on this point, the case was reconsidered. Thus two cases sent in in which typhoid had been diagnosed were doubtful

* The general mortality of the Hospital for the whole year 1892 was 6.2 per cent.; and on the medical side alone, as Dr. Wilde has found, 11.6 per cent. The excess of 14.50 per cent. was due to the fact that all the urgent medical cases come under my care and to admission of many patients *in extremis*.

and consequently set aside. Again, no case was discharged until the recovery was complete.

The diseases of the *Respiratory System* presented very good results. The recoveries amounted to 13. The cases included 4 of lobar pneumonia, 6 of acute pneumonia of severe type, and 3 of severe bronchopneumonia. To these ought to be added a case of extensive pulmonary congestion, which for a few days caused considerable anxiety, but which in the end did well. In another pneumonic case emphysema complicated, but ended well. The bronchial cases did equally well with the pneumonias; 5 acute cases made rapid and good recoveries, two of them being well-defined examples of the capillary variety of the disease, and one an instance in which an old-standing emphysema existed as a complication. Emphysema itself presented two examples of what seemed to be entire recovery, although in each case, when it first came before me, the emphysematous condition was well marked, and extended over a large surface of one lung. In both patients improvement in the general health and disappearance of dyspnœa accompanied the local improvement, but whether the local affection took precedence towards the recovery it is not easy to say. In a long experience of thoracic disease the removal of emphysema is to me almost a new fact in clinical observation. A case of hydrothorax and two cases of acute hæmoptysis made excellent recoveries.

Diseases of the Organs of the Circulation.—As would be detected from the description above recited, the cases of disease affecting the organs of the circulation were mainly of the chronic and incurable type. The acute affections of these organs were in most instances associated with rheumatic conditions, with which I

shall consider them. Some particular cases will, however, be referred to in dealing with the results described under the heads of "Relieved," "Unrelieved," and "Deaths."

Diseases affecting the Alimentary System.—The diseases in this order which ended in recovery were rather numerous, and offered features of great interest. They included 3 cases of gastric ulcer, 2 of acute gastritis, 1 of gastric catarrh, 1 of enteritis, 1 of gastro-enteritis, 1 of colonitis, 5 of dyspepsia, 3 of infantile diarrhoea, and 1 of chronic dysentery. They were altogether the most favourable and rapid recoveries, and they have impressed me strongly as proofs, not simply of the value of abstinence from alcoholic fluids whenever there is inflammation, irritation, debility, or relaxation of the alimentary surfaces, but, by comparison, of possible injury inflicted in such cases by the administration of alcoholic fluids. I remember quite well the time when, according to custom, I should have prescribed alcohol in all those cases that were not actually inflammatory; but I never remember having seen such quick and sound recoveries as those which have followed the non-alcoholic method. In two or three instances, such is the force of habit, I had great difficulty in preventing myself from falling back upon the old system, a kind of conscientious doubt, begotten of fear, coming over me. Nevertheless I held on in the new course, and with encouragement from results, so that I have occasion for satisfaction and confidence in the newer, simpler, and much more rational practice.

Diseases of the Nervous System.—Under this head there were 6 cases worthy of special regard, namely, 1 of cerebral hæmorrhage; 2 of cerebral symptoms

seeming to depend on syphilitic disease; 1 of hysteria; and 3 of chorea. The two first of these examples were very serious, and held out little hope of recovery, although in the end they did well. I cannot resist the belief here again, speaking from general experience, that the non-alcoholic method was as sound as it was successful.

Rheumatic Diseases.—The cases in which rheumatism played the leading part were rather numerous, as might be expected from a community in which rheumatic affections are singularly prevalent. The order includes 1 of rheumatic pericarditis; 1 of rheumatic endocarditis; 1 of rheumatic endo-pericarditis; 4 of acute rheumatism; 6 of subacute and chronic rheumatism; and 1 of subacute symptoms with mitral disease. There were also some minor cases; 1 of lumbago and 1 of rheumatoid or rheumatic gout. To these must also be added an instance of disease almost unique in character, namely of rheumatic arthritis affecting the occipito-atloid joint. Dr. Wilde, who received and reported to me on this case, came to this diagnosis, and although at first it rather astonished me, I had to confirm it. The symptoms were severe and anxious; but recovery in this, as in the other cases noted, was rapid and complete.

Enteric Fever (Typhoid).—In the 5 examples of this kind, each one presenting all the characteristic features of the disease, there was recovery of the best sort. They were very anxious cases, and I am certain that a time has been present in my life when I should have administered alcohol in each case, under the conscientious idea of its absolute necessity.

Cases of Acute Poisoning.—Of 4 cases of acute poisoning by swallowing corrosive poisons 3 recovered.

One of these was a most remarkable recovery. A woman, for the purpose of suicide, swallowed two fluid ounces and a half of a solution made by pouring commercial hydrochloric acid on solder, in the way adopted by plumbers when, as they say, they "kill the spirit of salt"—the acid. The injury inflicted was extreme: the lips, mouth, and throat were excoriated, the stomach at first rejected all that was swallowed, and there was intense gastric pain with signs of collapse. Recovery was scarcely expected, and the temptation was strong in me to administer an alcoholic stimulant by enema. The temptation was again resisted, and with every cause for satisfaction, for a better recovery could not have been wished for. The patient left the hospital entirely well, after having been kept in the institution for some extra days in order to be quite sure of her completed convalescence.

Miscellaneous.—Amongst the miscellaneous cases ending in recovery, I stay only to refer to one of diphtheria or membranous croup. It was in a female child, who was brought in just in time to be saved by tracheotomy, performed promptly by my colleague Dr. Collins. Relief of dyspnœa was obtained at once, and perfect recovery followed.

CASES DISCHARGED RELIEVED.

The 44 cases discharged as relieved were, with one or two exceptions, specimens of chronic diseases of the heart, the lungs, or nervous system, all in an advanced stage, for which relief alone could be anticipated. They included 3 examples of chronic phthisis, and 1 of fibroid phthisis; these were discharged much relieved, one so much that, but for a fear of error of hope, it would have been and might have been added:

to the list of recoveries. The list included also 2 cases of *tabes mesenterica*, one of which might also have been put on the recovery side but for the reason just given. To them should be added 1 case of locomotor ataxy, in which there was so much improvement that the patient expressed himself to me as feeling able to resume his employment. The case of pertussis noticed in the list under the head of "Relieved" was discharged before actual recovery as a case unfitted for the institution and an accidental admission.

CASES DISCHARGED UNRELIEVED.

There were 11 instances of this character, including examples of dorsal caries, of cirrhosis of liver with pulmonary emphysema, of carcinoma of the uterus, of locomotor ataxy in an advanced stage, of lymphosarcoma, of obscure pelvic growth, probably malignant, and of exophthalmic goitre. This last-named case was long under our care, and resisted every method of treatment. The left eye sloughed, and, at my request, was eviscerated by Dr. Collins, with satisfactory healing afterwards. The right eye then became affected, and, as all the resources of our art seemed to fail, the patient, a woman of middle age, returned to her friends, and, as we learned—for she was kept under observation—died ultimately from asthenia and enfeeblement of the circulation. In one or two instances patients classed under this list of "Unrelieved" were discharged by their own desire.

CASES ENDING IN DEATH.

Twenty-nine patients out of the 200 died; 2 were sufferers from acute pneumonia, coming into hospital at an advanced stage; 1 was a man suffering from

pneumonia with alcoholic symptoms, who died ten hours after admission in a condition of cyanosis, with which he entered; 1 was an instance of double pneumonia, also advanced on admission; 2 were examples of obstruction of the pulmonary artery from fibrinous deposition; 1 of acute pulmonary tuberculosis; 1 of rupture of an aneurism of the descending aorta; 2 of cancer of the large intestine; 1 of cancer of the rectum, with obstruction of the bowels, perforation, and peritonitis, in which the operation of opening the abdomen and relieving the intestine was ably performed by Dr. Collins; 1 was the suicidal case already referred to, in which corrosive sublimate was the poison employed; 1 was a case of diabetic coma, and the others were from the first in an equally hopeless state.

LINES OF TREATMENT, GENERAL AND SPECIAL.

The leading facts bearing on the recoveries, reliefs, negatives, and deaths having now been stated, it will be expected that I shall offer some account of the treatment, *minus* alcohol, under which those results have been obtained. Whether the results are good or bad I leave for others to determine; it is my duty simply to report them faithfully, together with the circumstances under which they occurred.

With the exception of the entire absence of alcoholic beverages within the walls of this hospital, the general lines of treatment do not vary from those of other carefully managed institutions of a similar kind. Our wards, large and well lighted, afford 1,500 cubic feet of space for each bed; the warming is from central Janus stoves, and the ventilation is free and as a general fact comfortable. Every possible attention is paid to cleanliness, neatness, and prettiness. We have abundance

of flowers and plants in the wards, with pictures and paintings on the walls. Our Grosvenor ward for children is a model ward from an artistic point of view, and the presiding sister and nurses take a great pride in keeping up its reputation in this respect. The temperature of the air of the wards is kept at a mean of 62° to 64° Fahr. Although the building stands in the midst of a thickly populated neighbourhood, it is free from loud and discordant noises. Around the wards are open balconies, wide and convenient, in which, in warm weather, the children who are fitted for it play, and in which the older patients enjoy the air and the sun, the sun being ever a welcomed friend. A large piece of open ground at the back of the hospital affords a good recreation-ground for all patients who can avail themselves of recreation out of doors.

The hospital until lately was artificially lighted with gas; but, by the beneficence of Mr. John Edward Taylor, it is now lighted with electricity, with the advantage to the professional staff of an electric lamp when required for each bed, a great advantage in diagnostic work. We are well supplied with ordinary baths; and, by the kindness of the Rev. H. Jones, we have been furnished with one of his cabinet hot-air baths. We want, however, very urgently a large hot-air or Turkish bath, with a vapour or Russian bath also at command, a want named not without the hope that some rich and generous man or woman will, when the important requirement is known, find the means for supplying it. Several patients I have had under my care would have benefited greatly by this addition to other methods of treatment.

Our nursing staff is very complete, under the direction of the able lady superintendent and the lady sisters,

with their complement of night and day nurses, to all of whom, as far as I am concerned, I owe sincere obligations. Our diet roll is simple, well prepared, and efficient. It is in four varieties: milk diet; middle diet, which includes light animal food, such as fish; full diet; and what we call, for particular cases, "fancy diet," a diet that is acceptable to and at the same time suitable for patients who can only take or digest special articles of food.

For medicinal purposes, we are as free as possible from all complexity. We use glycerine for making what may be called our tinctures, and in my clinique I am introducing a series of "waters"—aqua ferri, aqua chloroformi, aqua opii, aqua quinae, and so on—to form the menstruums of other active drugs when they are called for. I also follow the plan of having the medicines administered with a free quantity of water, and with as accurate a dosage as can be obtained, for I agree with Mr. Spender's original proposition that the administration of medicines in comparatively small and frequent doses is more effective and useful than the more common plan of large doses given at long intervals.

I treat many cases by inhalation, and for this end I use oxygen in a new and, I hope, efficient manner. I make oxygen gas a medium for carrying other volatile substances that admit of being inhaled with it. The mode is very simple. I have had a small table constructed that runs easily and quietly on casters, from bed to bed, and that stands, without taking up much room, by the bedside. On this are two or more Wolff's globe bottles, so fixed as to be firm and yet easily liberated by means of a little slide over their base. The bottles will hold each two pints of water.

There are two mouths to the bottles, one at the top, the other at the side. Through the mouth at the top there passes a glass tube, which descends nearly to the bottom of the bottle, and which, from its upper part, is connected with a tube running to the reservoir of oxygen. From the mouth at the side of the tube there proceeds a long india-rubber tube, nearly an inch in diameter, and connected at its free end with a soft cellulite, double-valved mouthpiece, which adapts itself neatly to the mouth of the patient, and has the great advantage that it can be washed as easily as if it were made of glass or porcelain, and can be used after washing by patient and patient without any fear of conveyance of infection.

In bringing this apparatus into use, we place the table on the right-hand side of the bed of the patient. We pour a pint of pure water into the bottle; and we add to the water in right quantity whatever volatile fluid—ether, ozonic ether, ammonia, amylene, amyl nitrite, or other agent—we may require. Next, we connect the tube that goes to the bottom of the water with the oxygen supply. Lastly, we connect the inhaling tube with the side tube of the bottle, and give the patient the mouthpiece at the free end of the inhaling tube to place over the mouth and nose. All being complete, we allow the oxygen to find its way through the water; it passes up in bubbles through the water, carrying with it the agent with which the water is charged, and so flows along the inhaling tube to the mouthpiece, to be breathed from it by the patient.

In my first essays in this employment of oxygen, I had the gas produced by treating a solution of peroxide of hydrogen with a solution of potassa permanganate. The oxygen supply was good, but expensive. I have

now replaced this method by having the oxygen sent to the hospital compressed in strong iron bottles by Brin's process. The iron bottle is connected with the Wolff's bottle by an india-rubber tube, and when the tap of the oxygen bottle is turned the gas is set free. The inhalation may be continued for twenty minutes to half an hour or longer, according to requirement, and there is no need to remove the mouthpiece, since respiration is readily carried on by the oxygen without any new admission of common atmospheric air. If it be necessary to warm the oxygen and the other agents introduced with it, that process can be secured by putting warm or hot water into the Wolff's bottle.

I use peroxide of hydrogen direct in my practice here as an internal remedy, the dose varying from one to four fluid drachms mixed with a free quantity of water. With the peroxide I often combine other remedies that unite with it in physiological action, and are compatible with it chemically. I use also frequently the ethereal solution of the peroxide ozonic ether, in doses of twenty minims to a fluid drachm.

SOME SPECIAL POINTS OF PRACTICE.

I am now brought to notice a few of the special points of practice which have been followed out in my clinique in the different classes of cases above recorded. In the pneumonic and bronchial cases the treatment has been of the simple and sustaining kind. The medicines that have been given during the acute febrile stages have been chiefly liquor ammoniæ acetatis and carbonate of ammonia in small and frequently repeated doses. The patients have all been well and carefully fed on the milk and middle diet until convalescence was declared. In some of the more extreme instances,

where there was fear of collapse from separation of fibrine in the heart or pulmonary artery, ammonia has been given freely according to the method I have for so many years inculcated. I have also in cases of depression, under which fibrinous separation is so easily developed, lighted on a mode of administering ammonia which combines feeding with the medicine. I direct that a three or five-grain tabloid of bicarbonate of ammonia shall be dissolved in a cup of coffee or of coffee with milk, and be taken by the patient in that manner. The coffee can be sweetened with sugar if that is desired by the patient, and the ammonia can be so administered without any objectionable taste to the beverage. After what is called the crisis in acute pneumonia, I administer very little medicine of any kind; I trust rather to careful feeding, with an occasional alterative or expectorant, as may be required.

In some cases of phthisis that have been under my care have been treated by the inhalation of ozonised oxygen. Since the first discovery of oxygen, the practice of employing it as a remedy in diseases of a chronic kind has been, intermittently, carried out. Hill produced results in the treatment of strumous patients by oxygen that were unquestionably of a remarkable character. The distinguished Chaptal tried the employment of oxygen in phthisis pulmonalis, and while asserting that it gave much relief in dyspnœa, was of opinion that it effected nothing in the way of cure. To use his own poetical expression, "it strewed flowers to the grave." Soon after I brought hydrogen peroxide into medicine, I administered it many times to consumptive patients under my care at the Royal Infirmary for Diseases of the Chest, as well as in private practice, and in a few instances with encouraging results. Later on, when I

produced ethereal oxygen, ozonic ether, I employed that also in phthisis, and with similar partial success. In these last months, since the oxygen apparatus for inhalation has been set up here, I have used ozonised oxygen as stated above ; and, in order that there should be no error in the matter, the inhalation has been trusted to alone. The oxygen has been ozonised by passing it through ozonic or iodised ether, and the inhalations have been carried on at least twice daily for intervals of twenty minutes to half an hour. The effects have been irregular, but striking. In two cases—of young women of the same age—in which distinct phthisical symptoms were present in the second stage of the disease, the inhalation was carried out in a systematic way. For some weeks both patients improved in a marked degree ; then in one of them cardiac trouble manifested itself with pyrexia, and finally pericarditis. The cardiac mischief became so severe, the ozone treatment was stopped ; special treatment adapted to the special circumstances was substituted, and the girl left the institution for a convalescent home. In the other case no complications arose ; and the patient, although reported as relieved on her discharge, was really well. All the symptoms were removed, she had gained in flesh, and when I last heard of her she was still in good health. In a third instance, very similar in all respects except that the tubercular lesion was more acute, the same treatment seemed to have no restraining effect, and the disease ran its usual course to a fatal termination. In another case, in which the disease was advanced on admission, much relief was obtained ; but nothing like a permanent recovery was presented. From these results I am strengthened in the view I have previously formed from

other experiences that the inhalation of oxygen in phthisis pulmonalis is manifestly useful in some forms of the disease, manifestly useless in other forms; and that these differences may occur when the method of administration is precisely the same. Much of the contradictory evidence that has been delivered on this mode of treatment is understandable by the explanation here given, and it is our duty in the future to endeavour to discover definitely those cases in which good service from oxygen may and may not be anticipated.

Other points of deep interest relating to the practices adopted in the treatment of the two hundred cases I have brought under your observation still lie before me, but I have already trespassed too long on your patience; and I shall only ask you to bear with me whilst I touch on the cases of enteric fever. They were five in number; and, as I have reported, they all recovered. They were typical illustrations of the disease of the purest kind; they were severe; and they occurred in patients of ages varying from nine to fifty years. Four were males, one a female. One was by trade a baker, another a gardener, a third a cook, and two were youths at school. There has been a period in my practical career when I should have administered alcohol in some form to every one of these patients; but, as they all made good recoveries without alcohol, it is quite clear that the employment of it, had it been carried out, would not have been a saving clause for them. Four of them were treated by the long-continued bath, after the method so ably developed by Dr. Barr, of Liverpool. One, a baker (aged fifty), was treated without the bath with peroxide of hydrogen in chloroform water, in doses of one to two fluid drachms of the peroxide in one fluid ounce of the

chloroform water. The medicine was further diluted with water for administration, and was administered as if it were a drink, but so proportioned that one dose was swallowed in the course of three hours. Under the influence of this medicine, or, not to be too presumptuous, during the administration of this medicine, favourable changes took place rapidly in this patient: the tongue became moist; the fever, which had risen to 104° Fahr., gradually subsided; the pulse fell; the tympanitis passed away; the mind became clear; and good sleep was, from some cause, induced. In two of the other cases the same medicine was given in the same way and, as it seemed, with similarly good effects; but, as these patients had also the benefit of the bath, I speak with less confidence respecting the efficacy of the medicinal remedy.

I now conclude this first review of practice as witnessed in my clinique and under my observation in this institution. I am satisfied that no aid I could have derived from alcoholic stimulants, as they are called, could have bettered my results. I have acted on the principle of doing unto others as I would they should do to me, and I feel sure any candid medical brother who will have the steady courage to put aside many old and unproven, though much-practised, methods, based only on unquestioning and unquestioned experience, and to move into these new fields of observation and experience, will, in the end, find no fault with me for leaving a track which, though it be beaten very firmly and be very wide and smooth to traverse, may not, after all, be the surest and soundest path to the golden gate of cure.

RAILWAY TRAVELLING AND HEALTH.

IN the old coaching days there was frequent complaint of the rheums and colds and exhaustions which befell the passengers who made long and wearisome journeys. The "insides" were stifled and crushed, the "outsides" were blinded with dust, soaked with wet, half killed with the heat, or frozen almost with cold. Only on bright and genial days in the better part of the spring, or in a mild and refreshing summer, or in the balmy autumn, was the coach ride endurable as a regular practice. We talk of the coaching days now with a sigh as of a departed glory. We look up at the old coaching-houses, turned, in these days, in many cases, into desolate wildernesses of bricks and mortar, with a feeling of sadness that such merry haunts of travellers should have come to such a sepulchral end. The ghosts of those who revelled in those old hostel yards and rooms still seem to haunt them, and the sharp notes of the guard's horn seem to reverberate once more as we stand in the desolate silence.

But if we could realise the actual advantages of those coaching days, and could separate the actual from the fanciful, we should not, I take it, feel that we had lost much when compared with what we had gained, while we should find we had got an immense number of advan-

tages which we had never before possessed. Quite apart from the rapidity of the railway system of locomotion, there is the shelter of the carriage in all seasons; the evenness of the transit; the freedom from pressure and cramped position, and the more sheltered accommodation which is at all times ready at all the great centres of lines and at most minor centres. On the whole, taking into account the numbers of people who travel and the distances that at all seasons of the year are traversed, there cannot, I think, be a doubt that in our days the mode of travelling by rail is less conducive to bad health than the old system, which it has so thoroughly and entirely superseded.

Let me not be understood, however, as conceding that the present system of railway travelling is *via salutis*, or an unimprovable course of treatment. Not at all. There are many dangers to health connected with the railway system, all of which, I believe, are removable, and some of which press in the most serious manner for immediate removal.

The dangers to be considered are of different kinds. Some are connected with the railway service. Others are connected with the travellers only, the railway system in itself having no necessary connection with the evils. Others are connected with the railway travelling, but are a part of the method from which there is no reasonable escape if this mode of travelling be adopted by the traveller. We may therefore recognise three classes of dangers to health from railway travelling, and may designate them:—

- (a) Railway dangers that are remediable.
- (b) Dangers from personal mistakes or errors.
- (c) Unavoidable dangers.

Under the first of these heads, the dangers to health

connected directly with the system itself and which are remediable, I connect the unnecessary noise and confusion which prevail at the station. I dare say a good many railway travellers, who are accustomed to the rail, will be inclined to smile at this criticism. I should be so inclined myself were I only a traveller by rail, for I have become so completely familiar with the railway station that it is really little more to me to get my ticket and place in a railway carriage than it is to take up my hat and get into a cab or carriage standing at my own door. The great majority of railway travellers are not, however, accustomed to the ins and outs of the railway terminus or station, and the fact, therefore, is that, to the great majority, the noise and tumult and anxiety are sources of very considerable nervous agitation, palpitation, and exhaustion. To children and to the aged the excitement and bustle cause an extreme shock which, in many instances, is not rallied from during the whole of a long journey, and often not for a considerable time after a journey has been completed.

The physical derangements which are established, during these bustling and noisy crises, are due to a series of nervous impressions made, in the first place, on the brain and organic nervous system, and by reflection on the heart. The senses are first perplexed; the sense of sight is not used to the best advantage; the sight is dazed. The sense of hearing is confused; questions are asked and answers given, in conversations which are not understood with the ordinary precision and clearness. Under these pressures the mind is confused and even the memory is rendered defective. It is almost impossible to get, at any time, into a carriage, in which there are many people, without hear-

ing one or more persons asking of others if the carriage is right for such and such a place. The inquirer may have asked to be shown to the right carriage half a dozen times ; as many well-informed officials may have correctly directed him ; the guard himself may have brought the traveller to the carriage, put him in and looked at his ticket, and yet as the train is beginning to move the traveller may—if there be the least hitch or hesitation, or calling out on the platform—ask, with the gravest anxiety, whether the train he is in is really going to his destination.

This perturbation is felt by those who in the course of a journey are obliged to take the chief command of others and see that all is right. It is not necessary that such a person should be at all weak-minded or nervous for him to fall into painful demur or difficulty. The fault may lie in deficiency only of a quick and resolute perception or perceptive faculty, or in the possession of what, at other times, is the best of all faculties, the faculty that thinks and reasons and that knows the importance of time for the due performance of both these processes. I have noticed that very thoughtful and cogitative men and women are commonly the worst of railway travellers at the stations ; if for a moment, they lend themselves to think and balance one probability against another, they are irretrievably lost and have to trust themselves to any guide who will assist them in their dilemma.

The causes of the confusion at the railway station and which are remediable are three or four in number. The first is the incomparable stupidity that enforces every person as he passes into a station to go through a kind of drill so that he may put his hand into a sort of rat trap in order to pay his money and get his ticket.

More than half the confusion in the station lies in getting the ticket, and proper change of money. If at respectable shops and depôts in the cities and towns where there are railway stations, tickets could be issued on small commissions, all the crush and separations of friends which occur at the station would be easily prevented.

Another evil is the unequal distribution of assistance at our railway stations arising from the hoodwinked permission that is granted to the servants and porters to attend first to those who can "tip," and to attend most to those who "tip" best. A shilling at a great station will put a man or family that has a shilling to spare in possession of a porter who will never lose sight of the man or family, who will keep ever so many together, mind the luggage, find the carriage, and lead the way to the most convenient places. Meanwhile those who either cannot or will not pay the extra fee, must keep themselves together, take care of their own luggage, find their own carriage, and look out at every step to see that they are right; in this the clatter and confusion, which make up so many difficulties, usually commence.

Another cause of mischief in the railway station is the din or noise, not of the people nor of the movements of the trains, but of the railway whistle. That shrill, piercing, deafening whistle from the engine during the time it gives forth its clamorous note is a very torture, even to the oldest traveller. Every moment it is inflicting its agonies it is preventing the travellers who, under pressure of time, are trying to make the best arrangements, from doing anything at all; and when the torture is over, a half minute or so is required to recover from the effect of the painful

vibration. The railway authorities ought to know what is best, but I think, if they would inquire into the positive necessity of this whistling torment, they would soon be able to discover an easier, more pleasant, and, at the same time, more certain method for attaining the good it effects without the evil.

The injuries to health which occur from the excitement and worry of the start at the railway station are at first transitory, and unless many times repeated, are not of necessity injurious in the strictest sense of that word. They consist of a series of shocks to the nervous system, inflicted, as I have already said, through the senses, and telling in a secondary manner on the heart and circulation of the blood. The heart at first becomes excited and beats more rapidly than it otherwise would, a condition which soon leads to a little confusion in the brain, and sometimes to a sensation of fulness and giddiness, with singing sounds and noises in the ear, apart and distinct from those which are produced in the station itself. In very nervous persons these unpleasant signs are followed up by a slight feeling of nausea, but, as a rule, all the signs pass away when the seat in the carriage is secured and the train starts on its way. In feeble persons, and especially in those who are suffering from feebleness of the heart, the effects of the temporary disturbance are not so slight. In these persons the overaction to which the heart has been subjected leads to weariness and failure of that central organ, and therewith to a feeling of fatigue and weariness which extends throughout the entire body, and which is commonly attributed, for many hours after the journey is over, to the fatiguing influence of mere travelling by rail.

In other persons, in whom the heart is fairly sound,

but in whom the digestive powers are enfeebled, the effect of the noise and commotion at the station is to bring on a very marked and painful attack of dyspepsia, —with flatulency, disturbed action of the bowels and kidneys, deranged appetite and depression of the spirits. This, again, as a class of symptoms of characteristic type, is often attributed to the journey rather than to the mode in which the journey was commenced.

Travellers who are subject to feeble or irregular circulation through the brain, and who are liable to giddiness and to vertigo from various exciting causes, are of all most affected by the noise and worry of the railway station. In their case the heart and stomach often share, sympathetically, with the nervous system, and they are apt to become giddy, faint, and nauseated. On these the motion of the train, after it has started, conveys sometimes a sensation of relief, which is continued until the train commences to stop; then the depression returns, and may be many times relieved and repeated in the course of one long journey.

Another remediable evil of railway travelling lies in the ventilation of the carriages, and in the arrangement for admitting and letting out air. Until recently this evil has been very great. The carriages were either too decidedly open, so that cold and sharp-cutting draughts were felt in any and every seat, or they were so badly provided with inlets and outlets for air that unless the windows were set open, and kept open, the traveller could get no air at all that was at once pure and free from draught. From these very bad conditions the carriages were rendered at one part of the journey so close as to be all but unbearable from the impurity of the air, while at another stage of the journey, when from absolute necessity the windows were opened, the

cold admitted was sufficient to make every one shiver, and to subject those who suddenly experienced the fluctuation from heat to cold to considerable danger. It is fair to say that at the present time very great improvements are being made on all our great railway lines to meet these sources of danger. The example set by the Midland Railway in the construction of their third-class carriages has been the most wholesome of examples. In these carriages the question of ventilation is solved by two improvements—the supply of a free amount of space in each carriage, and the plan of giving an open space above head in the centre of the carriage, into which air is abundantly admitted from well-arranged ventilators in the side walls. For the sake of the experiment, I travelled not long since from Edinburgh to London in one of these carriages. The carriage was completely full of passengers, and had in it as much luggage as could be comfortably stowed away. But so good was the ventilation that, without any reference to window ventilation, the air was free and fresh in the whole course of the journey, while there was at no time the slightest indication of inconvenience from draught or cold.

The dangers that arise from close air and draughts in the badly ventilated railway carriage are those of cold, congestion of the lungs, and feverish excitement. In these carriages also, children, and others who have recently been subjected to infectious disease, are most liable to become causes of infection to others. I have known many instances of infection of whooping cough and of scarlet fever in the railway carriage, and once I knew of small pox being communicated under the same conditions. In all these instances the carriage has been small, crowded, and ventilated, not from free openings,

above the sitters, but from the windows by currents of air which traversed the compartment from side to side, and which thereby conveyed the emanations from the breath and the clothing of one person to the air that was breathed by the others.

Another source of remediable danger in railway travelling is the vibration of the carriage, and the motion that is almost imperceptibly conveyed thereby to the sitter. In days when the seats of carriages were made of hard wood, and when no soft covering of any kind intervened to break the vibration, there was a great deal of mischief inflicted from vibration, together with muscular pain and cramp, caused by the hardness of the seats and the rests for the back. In the present day the introduction of stuffed seats, and, in some cases, of spring seats, has very greatly removed the evil of which complaint is here being made. Even now, however, travellers who are frequently and for a long time on the rail at one sitting are subjected to inconvenience, to say the least of it, from vibration. They describe that after sitting for a few hours they experience a sensation amounting, it may be, to slight pain along the whole course of the spinal column; and they say that when this is continued for a few hours it leads to a feeling of giddiness and nausea which is actually distressing, and which does not subside until long after the journey is concluded. One gentleman who consulted me professionally on this subject, assured me that when the vibration was long and distinctly felt, the result was to cause, not only the slight giddiness and nausea to which I have referred, but a temporary loss of power in the lower limbs, so that when he got out of the carriage and on to the platform he had a degree of uncertainty in his walk, and a feeling of

numbness extending along the lower limbs, which did not pass away until after a night's sleep, and which frequently caused him to wake in the night. In this gentleman, who was a regular traveller by rail, these symptoms increased so much that I was at last obliged to recommend him to leave the mode of life in which he was engaged and to follow a pursuit in which railway travelling formed no essential part. His freedom from the exposure to the vibration of the carriage was attended with rapid and permanent recovery.

In a less pronounced form I have seen other examples of a similar character to that above quoted. The cause of the evil would be best prevented by more careful stuffing of the seats, or, better still, by the suspension of the seats on springs. What is wanted in railway carriages is the introduction of seats made after the manner of the recently invented steel spring bed. This introduction would be at once a luxury and a benefit to health, for the softly stuffed seat, however easy it may be at first, is liable to become firm and uncomfortable after a time, while it harbours dust and other uncleanness. In third-class carriages, if in them cheapness as well as durability of construction were a great consideration, the seats could be set on springs arranged after the manner of the garden seat, which is made out of light bars of strong wood. Such seats would be as healthy as they would be easy and luxurious. From them there would be little vibration.

The second series of dangers to health in railway travelling, are of a purely personal kind, for which no care or assiduity on the part of railway companies could offer any safeguard or protection.

The first of these dangers, and, in respect to its immediateness, the greatest, is the haste which persons

who are going by train are apt to make on their way to the station. Time, tide, and train wait, they say, for no man ; so in order to cheat the time and catch the train haste in the act of catching is the art that is too often followed. There are two reasons for this. One of these reasons is that there are thousands of persons who by habit are always late in everything they do and undertake to do ; and the result of their habit in regard to train-catching is that they are almost invariably late in their setting-out. Another reason is that there are also thousands of persons who, under the name of active men of business, never procrastinate, but by rapidity of movement make it possible to get through more work than any man is naturally destined to perform. The great Dr. Priestley said of himself that he could never be blamed for putting off until to-morrow that which he ought to do to-day, but that he might often be blamed for doing to-day that which he ought to do to-morrow, and this is true of the class of men I mean. They plan everything to the moment, and if all goes smoothly in the work of the day, all may go well. If all does not go smoothly, if there should be a little delay here and a little delay there, it is, in their minds, essential that time should be, in some way, made up. They hurry therefore through parts of their work which do not require thought, and nothing is more tempting for hurry than the journey to catch the train. With their hearts already worn from work, off they go for one spurt more, to cheat the time and catch that iron sprite which cares for no man.

Both these sets of travellers are voluntary sufferers to railway travelling. The first class of them is always well supplied with men of lax fibre and feeble heart. The second class is always well supplied with men whose

hearts are at all times, during long business hours, acting at high tension, beating away at full speed and quickly exhausting the nervous powers. Both classes yield equally well the precise class of man who is not in the condition to catch a train. If you were to set either of them to catch a mouse, they would feel breathless at the very proposal, but the train is quite a different thing.

Many, many times a year the newspapers record the sudden death at the railway station of men who thus hurry to their doom. Their deaths are said to be sudden, but if the medical men under whose care they were and who knew them best were asked, they, as witnesses, would not express much wonder at the suddenness. These witnesses would tell that the persons in question had been suffering for months from difficult circulation; had been warned, time out of mind, by the medical adviser, of the danger of sudden excitement; had been forewarned, in the clearest manner, that the elastic element of their bodily fabric was not in the condition it once was to bear strain and re-act; and that the end was precisely that which had been, or might have been, positively foreseen and predicted.

The newspaper record of sudden deaths from haste to reach the train, is, moreover, a very small part of the greater record which it supplies to those who read beyond the immediate and startling event which it chronicles. The fact is that the haste is in a great number of cases the inducing as well as the immediate cause of the catastrophe. The strain that has oftentimes been put on to accomplish the feat, has been the accident, or the series of accidents, that has led to disease in the organs of the circulation; and as the feat has been often completed without anything more than

a temporary breathlessness, the escape from danger has been accepted as a sign of endurance ; until at last something that might have happened long before brings the inevitable and fatal conclusion. There are few acts of a physical kind, performed by men in the regular course of life, which so demonstrably produce physical disease of the heart and great blood vessels as this one act of running or hastening to catch the train. There is more in the act than the mere haste ; there is the attendant excitement, the looking out for the signs that the train is still there ; the breathless mode of inquiry how long it will be before the train starts ; the hurry to get past people on the platform ; the trepidation, perchance, to get a ticket ; the numberless little irritations which come in the way ; the pits and pats of temper, if any obstacles give trouble and annoyance ; and other anxieties which I need not wait to describe. These all add their quota to the general disturbance, and to the strain which cripples or to the strain which kills.

The evils arising from haste to catch the train are most frequent in persons who have passed their fortieth year, and, as a rule, they are brought about by physical injury affecting either the heart or the great arterial blood vessels which spring from the heart. The heart becomes enlarged, dilated ; the openings to which its valvular appendages are attached become too large ; the valves themselves become distended and pouched ; the great arterial vessel which rises from the left side of the heart becomes dilated and loses its contractile power, or the muscular walls of the heart become deficient in tone. The heart affected in any of these ways may work under ordinary circumstances so well that it may give to its owner no indications of its failure

unless it be severely taxed. It might, indeed, go on working, under fair circumstances, for years, and carry its owner into the possession of a long life if it were not overtried. But then occurs one of these violent strains, and therewith it succumbs.

In addition also to the faulty mechanism of the heart itself, in these examples, there is another influence at work which helps to increase the danger. When the nervous system is under excitement, the nervous control over the circulation of the blood in the minute blood vessels of the peripheral or extreme surfaces of the body is deranged. Those vessels at first are very much contracted, so much so that the hurrying person looks pale and appears breathless. The labouring heart is resisted in its action by the contracted condition of the minute vessels at the extremities of the circulation. The blood cannot pass freely through the vessels. It may happen that at this very stage the heart may fail. Or another stage may be reached. Under elevation of animal temperature and powerful cardiac pulsation, the restricted vessels may relax; then the body becomes heated and glowing, and the heart beats more freely. Here, however, again is danger, for now the heart overworked, and, exhausted from excess of strain, may cease from that cause; or a weakened vessel, over distended, may give way under the same circumstances.

The accidents from hastening to the train are, as I have said, most frequent amongst those who are at or over middle age. They may be inflicted earlier in life. A medical student I knew, in spite of the most earnest warnings, would insist in running from his school, after a certain afternoon lecture, to a train that carried him home to dinner. He kept this up for some months, with

the effect of producing a considerable increase in the size of his heart. This was followed by embarrassed breathing and by an acute congestion of the lungs with spitting of blood. In the end he became consumptive and died of pulmonary consumption, concerning the immediate origin of which there could be no doubt in the minds of those who had watched the career of the sufferer from his first to his last indications of an organic disease, to which he was hereditarily disposed.

If it be thought that, in naming these illustrations of mischiefs from causes connected with railway travelling, I am writing what may give alarm, then I am doing what I meant to do. I am only stating the simple truth, and as my mission is to prevent disease and death, I want to forewarn those who are rushing ignorantly and madly into both, of the dangers that are before them. The danger is now so often incurred I cannot make matters any worse by declaring it, while I may largely influence in the opposite direction—namely, towards health and safety—by the declaration.

OPUSCULA PRACTICA.

"There are mites in science as well as in charity."

BENJAMIN RUSH.

PHENOMENA OF UNCONSCIOUS EXISTENCE.

PERSONS of both sexes—they can scarcely be called patients, in the common acceptation of the term—sometimes consult us physicians respecting phenomena the description of which is not met with in the text-books of physic, and perhaps not in books more erudite. Still these consultations are now and then as practical as they are curious. One of these relates to the subject of unconscious existence. A preacher very eloquent in the pulpit, earnest in his parochial duties, and altogether so closely occupied, he had "not a minute to call his own," came to me once in considerable anxiety of mind. "I am," he said, "in such strong health that my people are always congratulating me on my robustness and activity, both in mental and physical life; I eat well, drink well,—as well as can be expected from a total abstainer from all intoxicants,—and sleep my eight hours every night at a bar; I am not troubled with dreams; I have no unusual anxieties of a domestic kind, and I am an actual stranger to pain. But, this alarms me: when I am preaching and am well into my sermon, an idea, without bidding, flashes across my mind that I have never left the pulpit since the previous Sunday; that is to say, I have lost all consciousness of the work

and events of the whole of the busy week that has passed away since I last preached to the congregation. Thereupon I continue to read my discourse more mechanically than I otherwise should, my mind being actually distracted, to a certain extent, in solving the question that has risen up before it. I want to know," he added, "whether there is any danger in this phenomenon. Is it what may be called a warning of some mental or physical failure?" To the inquiry thus made I replied that it was an undoubted warning, that certain parts or centres of the mental organism were being overworked, and that there was risk of wearing them out, upon which it were not easy to predict what the result might be. It might be something like taking away or weakening a part of the wall of a dwelling-house; and it would be wise, although the danger was not now pronounced, to take not a rest, but a change of mental work, a transfer into new mental scenery, a journey into new lands, new labours, and new books. The advice was followed, with the best of results; and I should recommend it to all who detect the same symptoms of unconscious passage of time from the same cause.

My friend's experience was that of a mind wearied in parts from prolonged work on one subject or one series of subjects; but there is another state of unconscious existence which is due to intensity of thought alone, not to repeated and monotonous strain. To one blessed, or unblessed, with imaginative powers, the unconscious existence of intensity is a marked phenomenon. William Blake—*Pictor Ignotus*—in one of his inspired passages says: "The world of imagination is the world of eternity." How true! and how he knew the fact is most perfectly told in a letter he

wrote to his friend Butts, from Felpham, in 1803. He is speaking of a poem he has just completed, and he adds, "The time it has taken in writing was thus rendered non-existent." The word "thus" is explanatory, from the context, of a theory the great man had formed respecting the nature of the phenomenon. His theory was that he had written from the dictation of some spiritual power which eye had not seen nor ear heard. The theory, incorrect as it is, was natural as coming from a child of imagination who knew nothing of physiology, or of the absorption, for a season, of all cerebral power by one centre, to the exclusion of vibration to other centres and to the cutting out thereby of other earthly things. It is no doubt a strange and sweet sensation, breathing eternity and mocking time. The hours have struck, as ever; the light has shone forth and become eclipsed by darkness; or light of day has stolen in from darkness; but there has been no record on the mental surface of any lapse of time: nothing has disturbed until the failing physical nature has cried out, "Fatigue! fatigue!" Astounding! it is night, or it is break of morning; a poem, a history, a calculation, a discovery, has been born into the world for goodness, nothingness, badness perchance, and all the time expended on it has been unrealised, a development from unconscious existence. Such unconscious existence were more serious than the prolonged work-a-day unconsciousness, except that it is more occasional, and that after it there is quick return from eternity into life with new or renewed associations. It requires nevertheless to be looked after, and suggests the necessity for repose.

FISH-POISONING AND THE DISEASE "SIGUATERA."

SPANISH colonists give the name of Siguatera to that union of symptoms which results from eating poisonous fishes indigenous to hot countries. The symptoms which arise are of two kinds: severe attacks of indigestion or gastro-enteritic poisoning; or, an icy coldness and depression, accompanied with great nervous disturbance. The symptoms are similar, varying in intensity only, whether severe enough to cause death, or merely to excite inconvenience or temporary derangement; they differ only, *i.e.*, in intensity. Gastro-enteritic siguatera has all the appearance of a severe attack of indigestion, *viz.*, nausea, vomiting—first of the food, then of mucus—coldness, depression of the pulse, cramp, and diarrhoea. The nervous types of symptoms—*viz.*, convulsion and paralysis—which characterise the process of poisoning by fishes, are not to be found in any case of metallic poisoning. They seem to arise from a combination of accidents, as if they had been produced by different vegetable poisons of narcotic and acrid character. When the siguatera assumes a gastro-enteritic form, the sufferer is, in general, quickly restored to health; while the nervous symptoms leave behind them the most serious traces of debility and irregularity which have been known to continue for eight or nine days.

There is, usually, no difficulty in distinguishing the siguatera of hot countries, the symptoms being well defined. The only difficulty that could arise would be in the similitude of the minor symptoms of the gastro-enteritic form and those arising from poisoning by copper or arsenic, and in the analogies of the nervous or convulsive form with the effects of vegetable poisons.

As illustrating the way in which the members of crews of vessels are poisoned by the eating of poisonous fishes, the following facts communicated to the late Sir William Burnet are valuable. The accident occurred on board the Dutch ship *Postilion*, lying in Simon's Bay, Cape of Good Hope. The *Winchester* being near, Mr. Jameson was called to render his services to the sufferers. On arrival he found that the boatswain's mate and purser's steward had been suddenly taken ill after eating a part of a well-known deleterious fish, common in Simon's Bay and called the toad or bladder fish—the Dioden. They had been warned that the fish was poisonous, but were resolved to try the experiment, the boatswain declaring that the liver was not poisonous, but a great delicacy. They had partaken of dinner at twelve o'clock; immediately afterwards they partook of the fish, and scarcely ten minutes had elapsed when the boatswain became so ill that he was unable to raise himself without the greatest difficulty; his face was somewhat flushed; his eyes glistened; his pupils were rather contracted; his mouth was open; his lips were tumid and somewhat blue; his forehead covered with perspiration; his pulse weak, quick, and intermittent. The patient was extremely uneasy and in great distress, but still conscious; he complained of pain from constriction of the throat, and appeared inclined to vomit. It was with difficulty he could swallow a powder with some warm water. His state quickly assumed a paralytic form; his eyes became fixed in one direction; his breathing was difficult and accompanied with dilatation of the nostrils; his face was pale and covered with cold perspiration, his lips livid; his consciousness and pulse failed, and in scarcely seventeen minutes after partaking of the fish he was dead. The symptoms exhibited by

the purser's steward were of a similar kind. He also died within twenty minutes of the time after he had partaken of the fish.

The quantity consumed between the two men was the liver of one fish; the entire fish measured only from six to eight inches in length.

Other similar examples have been recorded by Proeger; in all death was rapid, and the effects were the same as in the instances above cited. It is worthy of note that some of the poisonous fishes named above are as hurtful to inferior animals as they are to man. Several illustrations of this fact have been collected. Dr. Collas, chief of the Marine Department of Health at Pondicherry, had occasion to inquire into the poisonous nature of the goby, as he had been informed by the Director of Police there that several accidents had occurred in a native Mussulman's family of three persons, who had partaken of a dish made of some small fishes called in Talmic *calonoulouvé*. The head of the family also told Dr. Collas that three fowls had died soon after eating of the same dish. A native doctor, or *mestris*, repeated this experiment of feeding fowls on the fish, and with the same result.

At eight o'clock in the morning Dr. Collas gave to one chicken three heads, and to another four heads, of these fishes: at half-past nine the symptoms began; at eleven o'clock they increased; between one and two the poisoned animals died, without convulsions, in a state of extreme prostration. In a second experiment, the bodies of these same fishes were used from which the heads had been taken off. The animals suffered from the same symptoms, but less severely, and were quite well the next morning.

The livers of ten gobies were administered to one

chicken, and killed it in two hours. The intestines of ten of these fishes, separated from the livers, produced the same results. The entire fishes, deprived of their livers and intestines, caused death in four hours and a half in other experiments. Three experiments made upon dogs, to which these fishes were given after being fried; the food caused the animals to be seriously ill, and deranged their digestion; but they very soon recovered.

I had the opportunity some years ago of meeting in consultation my late much-esteemed friend Dr. Alfred Carpenter, of Croydon, in a case under his care which presented all the characters of *siguatera*. The patient was a clergyman, strong, active, and of middle life, who one very hot day partook freely, at the counter of a well-known place of recreation, of lobster. He noticed nothing peculiar in the taste of the food, but soon afterwards became faint and nauseated "with the taste of lobster strongly rising" in his throat, accompanied with a sensation of "acid burning." This was followed by severe spasmodic pain in the bowels, diarrhoea, and all the symptoms of the acute gastro-enteritic disorder described above. For some hours we feared the worst consequences, but fortunately recovery took place, with a rather long convalescence owing to frequent return of abdominal pain and diarrhoea. We attributed the poisoning in this case, and I have no doubt correctly, to the lobster, of which the patient had partaken very freely, and some portions of which were ejected undigested by the mouth, others by the intestinal canal.

CYCLING FOR WOMEN.

IN many essays and papers I have dwelt on its effects of cycling on female riders. I refer to the subject again now because the interest in it—if I may judge from the many anxious communications made to me about it—increases, although I doubt whether the number of lady riders increases proportionately. The first question that is usually put on this topic is the kind of cycle that is best suited for ladies, the bicycle or the tricycle. At one time the answer in the affirmative was with the tricycle; now, I think, it lies decidedly with the bicycle. Bicycles are brought to such perfection, they are so low, they run with so little friction, they are mounted so readily, and are so graceful as machines, they hold unquestionably the first place. It is easy also to learn to ride them when the learner is properly taught from the first, and it has seemed to me that the art of getting the balance—which is the prime art—is more easily attained by the woman than the man. The readiness with which ladies mount and dismount is quite a lesson in ease and gracefulness, especially on those machines which are provided with a seat in lieu of a saddle. There is another and important advantage in the bicycle—namely, that in riding it there is less vibration than from the tricycle; indeed, when the machine is well fitted with the pneumatic tyre, vibration is practically excluded. Lastly, the dress is better arranged on the bicycle than the tricycle; there is less risk of the folds of the dress being caught in the wheels, and less resistance from the wind.

A second question relates to the amount of exercise

that women can take in cycling. This is a question very difficult to answer, owing to differences of training, practice, age, and constitutional peculiarities. Some women are more fatigued by ten miles' cycling than others are by fifty. The golden rule in this matter is to let sense of fatigue govern the amount of work done. Of one thing I am most certain : that as amongst men there is nothing saved, but everything lost, in fighting against actual weariness so the fact is still more marked in women, and that it is the safest of safe procedures for a woman never to carry her work on the cycle to a degree of fatigue calling for more than one good night's rest for recuperation. Altogether, I am of opinion that twenty-five miles a day is a thoroughly good day's ride for even an accomplished female rider on a moderately fair, ordinary road. It is good for women, as it is for men, to dismount occasionally and walk, and it is always good for them to do so when they are climbing long and steep hills. The change of movement brings new sets of muscles into play, and saves strain on the muscles of respiration.

A third question has reference to the age at which girls and young women should commence to cycle. I am inclined to the view that for children of either sex much cycling is not good. Whilst the body is rapidly developing it is easy to produce distortion of trunk or limb by any forced and constrained position, and the skeleton runs extreme risks until it has attained a certain fair degree of firmness of structure. Girls should not begin to ride regularly until they have reached their seventeenth year, and not then unless they are strong and well formed. In training, and ever afterwards, they should be taught to sit straight up on

the seat or saddle, and always to have the dress perfectly free around the waist and chest. The ankles ought also to be free, and the dress sufficiently short to prevent embarrassment to the movement of the feet. I can have no hesitation as to the kind of seat that should be used by women; the cushion seat is infinitely preferable to the saddle.

One more amongst many other questions must be briefly noticed, I mean whether there is anything anatomically or constitutionally in the woman that specially excludes cycling as a recreation for her. There is nothing more in this respect than in respect to horse-riding. Every competent medical man is able to discover and explain some states and conditions in which it were best for the woman to ride neither a cycle nor a horse; but all conditions being normal, women can indulge in the exercise just as safely as men. It is indeed of great use to healthy women for them to cycle. It secures a quick and sure cultivation of the senses; it leads to a good and healthy muscular exercise; it causes a fine expansion of breathing; it causes the lungs to inhale pure air; it quickens the circulation; and it brings to the mind a free and wholesome change of scene, which is a tonic of tonics to the depression incident to sedentary monotony.



THE PRECISE ADMINISTRATION OF ANÆSTHETICS.

IT has been inferred from some of the observations in the last volume of ASCLEPIAD on the subject of the cause and prevention of death from chloroform that no importance attaches to methods of administration of anæsthetics, and that

the body of the patient is itself the best regulator, use whatever mode we may, as if it were implied that whatever charge of vapour the patient can inhale, that he can endure. This is a misapprehension; and I am glad it has been pointed out to me, in order that I may correct it. I know there are peculiarities in individuals which modify action. The temperature of the breath of different persons is sufficient to produce a difference; the depth of inspiration of different individuals is sufficient to make a difference. I am sure that the different nervous susceptibilities of different individuals are sufficient to make even a vital difference; and I feel certain that some very sudden deaths attributed to anæsthetics are not due to the vapour—for if they were, deaths from the accidental inhalation of the vapour would be of constant occurrence amongst the makers and vendors of it—but to the syncope of fear occurring in the sick about to undergo operations. I think and believe that all these matters deserve consideration in estimating the facts of mortality from inhalation of anæsthetics, but this does not prohibit the most careful study of the precise modes of administration. I have always felt that we who were first in the field began to administer too recklessly, and in 1848 I was occupied in manufacturing an inhaler that should regulate and economise the process of inhalation, an occupation to which, since that time, I have often returned. I remember well also the different kinds of apparatus that have been invented by Sibsen, Murphy, Glover, Snow, Charrière, Duroy, Junker, and many others, and to Charrière's plan I had a very strong leaning for a long time. It seemed a most happy method of supplying the vapour, of chloroform especially, in an equable and graduated quantity.

It has also seemed to me to be serious matter of regret that what some call the exigencies of practice have stood in the way of scientific mastery of administration. The administrator of an anæsthetic has in nine out of ten cases far more responsibility at the moment than the operating surgeon. Yet this fact has ever been systematically ignored; the "chloroformist" has been held to rank as the mere assistant of the surgeon, and the art of administration has, consequently, not passed properly as yet into a science. In time this may all come right. Snow, who was by nature a pure man of science, always hoped it would do so, although, in his own case, the wish, as he said, was but the father to the thought.

There are certain foundations laid in regard to chloroform administrations, however, to which it were wise to refer, because in regard to this one anæsthetic at least, always a popular one, they apply with much force. These were first laid down by Snow, and will be found in his posthumous work on chloroform which I edited. I spent much labour also on the same subject, and I venture therefore to place the calculations as if they were postulates. (1) Thirty minims, or ten grains, by weight, of chloroform yield 7.6 cubic inches of chloroform vapour absolute. (2) One hundred cubic inches of common atmospheric air at a temperature of 40° Fahr. will take up and hold in solution seven cubic inches of the vapour, yielding a compound atmosphere of 100 cubic inches of air and seven of chloroform vapour; but at a temperature of 65° Fahr., which may be taken as a common temperature of the operating room, the 100 cubic inches of air would take up nineteen cubic inches of the vapour, and at summer heat thirty-five cubic inches of the vapour,

or as much as would be represented by a fluid drachm and a half of chloroform in liquid. (3) To induce the degree of narcotism in which, under chloroform, a surgical operation can be painlessly performed on an adult healthy person, eighteen minims of chloroform require to be absorbed by the blood.

The problem to be solved on these simple data is how to administer eighteen minims of chloroform by the lungs so as to secure complete anæsthesia. I do not see any mode of administration that can secure this object absolutely, but apparatuses have been invented that are meant to assist in attaining that precision. Snow invented a balloon which allowed the vapour of chloroform to be diffused, as he believed equally, in the proportion of four parts of chloroform to ninety-six of air; Clover introduced the bag that goes by his name for the same purpose; Chanière, as I have already said, invented a very good instrument; and after the introduction of methylene bichloride Dr. Junker constructed a plan on the principle of Wolff's bottle by which the vapour of methylene or chloroform could be blown over into a mouthpiece by means of a pair of small bellows. Junker's apparatus has met with much favour; it is simple, and is based on a sound principle. I have used it for methylene ever since it has been before the profession, and with great satisfaction. Of late also it has been much improved, by its manufacturers, Messrs. Krohne and Sesemann, in the adaptation to the mouthpiece of a feather valve by which the movements of the respiration of the patient are beautifully indicated, and by the further addition of a long ear tube from the mouthpiece, through which the sounds of breathing can be heard by the administrator. Everything here conduces to the detection immediately

of any error of respiration, an arrangement which seems to me of immense advantage. Bearing upon the effectiveness of this apparatus, some calculations that have recently been made regarding the quantity of chloroform passed over into the mouthpiece, under the action of the bellows, are, I think, worthy of notice.

Provided the administration of the narcotic vapour is well timed and the vapour is inhaled with the normal respiration of an adult, it is found that at the close of the first minute about twelve and a half minims of chloroform have entered the lungs, and, on account of the smallness of the dose, are probably all absorbed by the blood, and are sufficient to induce the second degree of narcotism of Snow. Continuing the inhalation of one minim of chloroform per bellows compression during the following eight inspirations, about twenty and a half minims will have been inhaled, or, allowing a loss of two and a half minims by exhalation or the faulty timing of the administration, eighteen minims will have been absorbed during the first one and a half minutes, sufficient to induce the third degree of anæsthesia, or the condition in which surgical operations are usually commenced. To arrive at an even result, however, the administration must be made in a temperature not lower than 65° Fahr. Higher temperatures do not seriously interfere with the conduction of normal anæsthesia, as greater evaporation of the anæsthetic is under control and can be lessened and regulated by blowing less air through the bottle; but if the temperature of the air and of the anæsthetic is below 65° Fahr., and if stronger vapour is needed, it can be produced by holding the bottle containing the anæsthetic in the warm hand.

A great number of deaths under chloroform have

occurred during the first, or first few, minutes of administration. This fact suggests that undivided attention should be given to the respiration by the administrator during the first minute, and that the administration should be commenced, in every case, with chloroform very much diluted, with systematic gradual increase to the strongest vapour the patient is able to inhale with normal and regular respiration.

In normal chloroform administration, the proportion of chloroform and air inhaled during the

First	5	inspirations	is	1	minim	chloroform	to	120	cubic	inches	pure	air.
Second	5	"	"	1	"	"	"	60	"	"	"	"
Third	5	"	"	1	"	"	"	40	"	"	"	"
Fourth	5	"	"	1	"	"	"	30	"	"	"	"

In other words, the administration commences with about $\frac{1}{2}$ per cent., gradually rising to, and not exceeding, about $3\frac{1}{2}$ per cent. of vapour to the air inhaled. Provided anæsthesia is not complete, the strongest dose is continued until satisfactory anæsthesia is induced.

It may be presumed that vapour so much diluted is freely inhaled and probably all absorbed. If this presumption be true, anæsthesia will be sooner induced, than if stronger vapour were given, which the patient may refuse to inhale and struggle against. Snow's rule that the second degree of anæsthesia is induced by the absorption of twelve minims of chloroform; the third degree of narcotism, or the degree in which surgical operations are usually commenced, by eighteen minims; deep anæsthesia by twenty-four minims; arrested respiration by thirty-six minims absorbed, may be accepted as practically correct.

The following are the results of some experiments made with the regulating inhaler, on evaporation and

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loss of temperature of chloroform, during the first and second minutes of its administration in small, continuous, and gradually progressive doses, one minim at each inspiration by the improved Junker inhaler:—

Temperature, 62° Fahr.; six drachms of chloroform in graduated bottle; twenty respirations per minute. One full compression of the bellows evaporated on an average one minim of chloroform, which could be diminished at will to the smallest fraction of a minim.

The evaporation of chloroform was found to vary greatly at different temperatures. The higher the temperature, the more vapour is given off. Thus the first complete compression of the bellows evaporates two minims, as the evaporation of the chloroform is favoured from the bottle having been handled and from some chloroform hanging on its side, from pouring in the chloroform.* At each stroke of the bellows the temperature of the chloroform falls lower and lower, and its evaporation is proportionately less and less.

To give chloroform systematically to an adult during the first minute, the

							[inspiration.
First	5	inspirations	should	take	$\frac{1}{4}$	compression	of the bellows at each
Second	5	"	"	"	$\frac{1}{2}$	"	"
Third	5	"	"	"	$\frac{3}{4}$	"	"
Fourth	5	"	"	"	1	"	"

About thirteen minims of chloroform are evaporated (it varies between twelve and a half and fourteen minims), with a loss of its temperature of 4° Fahr.

* This fact points to the necessity of commencing the administration with very little pressure on the bellows, so that at first only a tenth or a twentieth part of its volume of air is made to pass through the chloroform; *i.e.*, it is the safest plan to give, with the first few inspirations, vapour much overdiluted, and gradually and systematically to increase its strength at each following inspiration.

If the first minute is followed up, without an interruption, with ten full compressions of the bellows, ten more minims are evaporated, or twenty-three minims during one and a half minutes, with a loss of its temperature of 6° Fahr.

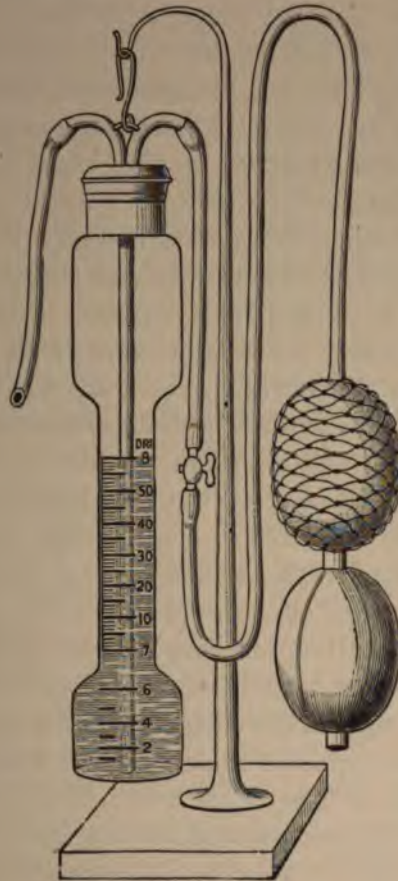
If the first minute is followed up with twenty full strokes instead of ten, seventeen minims more have been evaporated, or a total of thirty minims at the close of the second minute, with a loss of 10° Fahr. in its temperature.

After complete anæsthesia is once induced, proportionately small doses given at each inspiration suffice to maintain it. Less air being then passed through the chloroform, loss of its temperature is less marked in proportion.

For the purpose of determining the precise amount of chloroform or methylene lost during an administration by the method above named, Mr. Krohne has had prepared a bottle for holding the chloroform, after the kind sketched in the diagram on next page. The narrow graduated part of the bottle enables the operator to see the loss of fluid minim by minim, and is a great improvement. In administration, it is advisable to carry the bellows over the shoulders; by that means the valve of the bellows is always in proper position.

The objection that may be made to the measured method of administration here described will probably be that in many cases it is a slow method, and that in the hurry of practice time is an object. I think, however, that, as it is a safe method, one that removes greatly the risk of the primary reflex, one that economises the anæsthetic, and one that does not surcharge the body of the patient and yet supplies sufficient saturation of the blood and nervous system to master pain, the

question of time should not alone govern the position ; and while I would not venture to affirm that all deaths from administration will be prevented by the adoption



of this method, I am clear that it is rationally scientific in its intention, and is sure to reduce largely the large mortality from anæsthesia which is now so unfortunately presented.



METHYLENE FOR ANÆSTHESIA.

WHILST on the topic of anæsthetics and anæsthesia I feel bound to notice an article in the *Medical News* of February 25th, 1893 (pp. 217-219), on "Methylene Bichloride."

As is generally known, I introduced this anæsthetic into medicine in 1867. The substance had been made by the chemists previous to this time, but it existed merely as a chemical curiosity. I was led to it by the experiment of combining chloride of methyl, a gas at ordinary temperatures and a fine anæsthetic, with anhydrous ether, in which it is soluble. The result led me to seek for a more stable compound, and this was found in the bichloride. It was a most pleasant vapour to inhale, and it differed from chloroform in composition in that one atom of chlorine of chloroform was replaced by one atom of hydrogen. It was CH_2Cl_2 , instead of CHCl_3 . I reported upon it at the meeting of the British Association at Dundee in September, 1867, and the report will be found in the *Transactions* of that meeting (pp. 46—57). I said of it that, "compared with other anæsthetics, bichloride of methylene appears to combine the anæsthetic power of chloroform with the safer properties of ether." On October 15th of the same year, Mr. Peter Marshall, of Bedford Square, administered the vapour of the bichloride to me up to complete anæsthesia, and after I came out of the narcotism I went from my house (12, Hinde Street, Manchester Square) to a lady under the care of Mr. Spencer (now Sir Spencer) Wells, and administered the vapour to her whilst he performed the operation of ovariectomy. The anæsthesia was perfect, not one unfavourable symptom being mani-

fested, and I believe from that day until the present Sir Spencer has loyally held by the anæsthetic.

In 1868 I reported further on the bichloride, and again in 1869. In the last-named article I made the following observations:—

“Bichloride of methylene differs from chloroform in action in several particulars. The anæsthetic sleep is produced more quickly, and when produced is more prolonged. On the other hand, recovery, when it commences, is far more rapid; indeed, the period of recovery, according to my experience, is never extended over four minutes, and there are no prolonged or painful after-effects.

“When animals are allowed to sleep to death in vapour of bichloride of methylene, the lungs are found containing blood, but not congested, while the heart contains blood on both sides. In this respect the vapour acts differently from both chloroform and ether. After death in chloroform vapour the lungs are left bloodless, and the right side of the heart gorged with blood. After death from vapour of ether the lungs are left intensely congested, with the heart containing blood on both sides.

“Bichloride of methylene holds a place between ether and chloroform. It is safer than chloroform, but not so safe as ether. In matter of efficiency of action it is equal to chloroform, while it is quicker in action, and more persistent, and far more manageable than ether.

“Bichloride of methylene has been largely used during the past year. Reports of its employment have reached me from New York, Paris, Hanover and other parts of Germany, from Australia, and from different towns in England. These reports are exceptionally

good, and no fatal accident has as yet befallen the administration.

"There is, however, as yet one drawback to its general introduction; I mean the difficulty of manufacturing it on a large scale at a reasonable cost. It is also, when pure, more difficult to keep than chloroform, its boiling-point being at least 33° lower on Fahrenheit's scale ($18^{\circ}3$ C.).

"The question of the exact composition of the bichloride of methylene has been recently determined from analysis by one of our best organic chemists, Mr. Perkin, who has confirmed the composition as CH_2Cl_2 . He has, however, made a correction of the boiling-point, which he places at 104° F. (40° C.). This is 16° F. higher than has been given to it by other observers, as well as by myself. Quite independently I had also detected this error, which has arisen, I doubt not, from the presence, in the earlier specimens of the fluid, of a little chloride of methyl. I think that even yet there is some slight cause of error, and that the boiling-point is actually 111° F. ($43^{\circ}8$ C.).

"The difficulty of manufacturing bichloride of methylene in sufficient quantities at a reasonable cost is, I repeat, the only objection to its more general employment. In quantities of two or three ounces the manufacture is easy; but the distillation at the low temperature, which is required, renders the process tedious on a large scale, and therefore expensive."

The article in the *Medical News* treats of methylene without reference to these preliminary data, as if the substance had come into use without any preparatory or original study of its action. It affirms, on the authority of a new writer on the subject, that there is a true and a false methylene bichloride, and that

the true bichloride is not an anæsthetic. Then it affirms that there are four different substances laying claim to the same appellation: (1) the preparation made by Robbins; (2) a mysterious substance of English manufacture which M. Reynault found to be a mixture of chloroform and methylic alcohol; (3) the real bichloride, of which the formula is " $C_2H_2Cl_2$," a useless anæsthetic; (4) a mixture of which nothing definitely is known.

Bearing on these four assumed varieties of methylene bichloride, I have to say that the specimen made by Robbins is, and always has been, made by the reducing action of zinc on chloroform. It is the true anæsthetic CH_2Cl_2 , and if in any specimen a small quantity of chloroform has been left undecomposed, that is an accident not of material moment. Of the second, or "mysterious" variety, I know nothing. I have sought for it everywhere with much anxiety to find and expose it, but without success. The third variety, said to have the formula $C_2H_2Cl_2$, is not a methyl at all; it must belong to the ethyl or ethene series, and cannot be a good anæsthetic with two atoms of carbon, two only of hydrogen, and two of chlorine. If it contained four of hydrogen it would be ethene chloride, or Dutch liquid, an anæsthetic which acts much like chloroform, causing a long spasmodic stage; but, with two atoms only of hydrogen, it would be practically worthless and, indeed, dangerous, a fact which any one who understands the physiological action of anæsthetics according to chemical constitution would at once predict. Of the fourth variety I know nothing. It, like the second, I cannot find. I do not deny its existence, for there are fraudulent drugs as well as other fraudulent things, and this compound may be of that order.

I turn now, for a moment, to methylene made by the reduction of chloroform; methylene, as I understand it; and I state, in regard to it, that, except for the accidental presence of chloroform, it is not a mixture. I repeat that it has a special physiological action, that it is more rapid in its action than chloroform, causes vomiting less frequently, and can be made to sustain a good anæsthesia for any required time. I maintain also that, while it may be more dangerous than ether, it is less dangerous than chloroform. It must also be related that in one case in which death was attributed to methylene the substance that was the actual cause was chloroform: chloroform had been put into an empty bottle marked methylene; while in another example the fluid supposed to be methylene was chloroform, as I found by an examination of a specimen of the fluid from the same store that supplied what had been used. How many deaths therefore have occurred from the true specimen it is impossible to say; but at first, when only one kind was used in England, the freedom from fatality under methylene was subject of general commendation. I see that in my report of 1869 I wrote, "Up to this date no accident has occurred in the administration of the bichloride. I have never held that it is free from danger, and I reported in regard to it, at first, that it could not be considered as the safest agent of its kind until it had been administered twenty thousand times with a totality of less than ten deaths as the result. I hope, however, now that it may even go safely through this very severe ordeal; for at the lowest estimate it has been administered four thousand five hundred times in England alone without one fatal catastrophe. In the experience which has sprung from the administration of this anæsthetic, one very interest-

ing and valuable practical truth has come forth, viz., that, with skill on the part of the administrator, the most rapid and safest insensibility can be induced with a quantity not exceeding sixty minims; thus for small operations—or, I had better say, short operations, such as tooth-extraction, operation for cataract, etc.—the required anæsthesia can be induced in from twenty to forty seconds, with perfect recovery in from three to five minutes. I may be pardoned for dwelling on this practical fact for one reason alone—for this reason: that it shows the true value of a correct theory in estimating the properties of chemical substances on living animal bodies, based on the physical characters, purely, of the substances themselves. From the chemical composition of the bichloride of methylene, from its boiling-point, and the density of its vapour, I was able to claim for it its probable place as an anæsthetic before it was subjected to the test of experience, and especially to claim for it that its power to take effect would be as rapidly developed as would be the recovery from it when it was withdrawn. The practice has proved the theory to have been in this case safely founded; and the fact may be accepted as an encouragement to all who are or may be striving to reduce our knowledge of the action of medicinal agents to fixed principles. For my part, I am certain the day is at hand when, the mere chemical and physical characters of any substance being known to the physiologist, he will be able to foresee clearly what are the physiological values of the substance, and to calculate the symptoms and changes it will induce in the animal economy from pure theoretical formulas and with perfect precision."

As a commentary on the above remarks, I may add to-day that since I first tested methylene on myself and

then administered it to Sir Spencer Wells's patient (October 15th, 1867) I have never employed any other general anæsthetic. I have administered methylene with the Junker inhaler to persons of all ages, for operations short and long, and for nearly every possible operation. I have never once witnessed from it an untoward result. This is just, and no more; for although the use of methylene is one of the experimental results of my own brain, I am not, and never have been, blind to its possible dangers, and I would cast it aside in an instant if a better agent were at my command.

CANDOUR IN PROGNOSIS.

THE author or authoress of the remarkable modern work entitled *Gossip of a Century* observes, in speaking of the relationships of doctor and patient, that "faith in his medical attendant and faith in his recovery are the sick man's staff, and will often save him when treatment fails; the greatest Physician told His patient in so many words it 'was faith that made him whole,' and every doctor who is worth his shilling, to say nothing of the gold coin, knows the power of imagination."

The opinion thus expressed is a lesson drawn from the observation of its writer on effects which have been supposed to follow the communications, in the form of prognosis, which sometimes pass between the patient and physician. The case of Balzac is quoted, and the idea is conveyed that "the too conscientious physician often takes away the patient's last chance. Let him (the patient) read the word 'hopeless' in the doctor's

face, and, however brave, he is lost ; a dose of poison would not be more effectual."

I look back over a long period of practical experience as I study these statements. I ask, Is perfect candour in prognosis really so bad ? The conclusion, the only conclusion I can arrive at, is that candour in regard to good or bad effects turns entirely on diagnosis. If, though symptoms may seem urgent, the physician be absolutely certain there is no organic change of structure in the sick man, change involving life, then prognosis of hope and persistence in favourable prognosis is a good and curative procedure ; and faith plays its splendid part with the greatest credit. But when there is sure and certain evidence of organic disease of vital organ or organs, then all the faith in the world will never make whole. It is in these last-named conditions that prognosis is so difficult and delicate a subject. It is at the moment when the physician is certain that declaring the worst can have no material effect on the condition of his patient that candid prognosis is thought to be so doubtful by lookers-on. But suppose in such a crisis the physician speaks hopefully and looks unconcernedly, knowing the deceit he is carrying out, what can he expect to get but merciless criticism when the near and inevitable event has occurred ? His whole reputation is at stake if he deceive a dying man and a dying man's friends.

Candour in prognosis is right, let the consequences be what they may, when it rests on assured diagnosis ; and so far from having a bad, it often has a good, effect. The foreknowledge of the worst brings to most minds, even the most timorous, resignation in place of despair. It makes way for preparations of various kinds ; and when it is properly led up to, it has nothing whatever

to do with curative faith. Faith never healed an aneurism, restored a cavernous lung, removed a fibrinous plug from the heart, brought back to natural function a malignant gland, or dispersed a stone in the kidney, ureter, or bladder; and when any final deathly arbitration is present, the wisest physician's word or look can be of no vital consequence to the fate in store for the sick man. On the contrary, firm and candid statements are the best that can be offered. I recall well the case of an important official of the State during a period of great political crisis. This gentleman was within a few weeks of death from an incurable disease. He was under the care of Dr. Daniel Noble, Sir Thomas Watson, Dr. Arthur Wilson, Dr. George Budd, and myself. His family pressed us "to hold out hopes of his recovery." We declined. Another physician was summoned who, entirely replacing us, held out such hopes that a public announcement of a speedy recovery and return to official duties went forth. In three weeks the death of that patient took place, by what might well be called natural necessity, and the confusion that followed, the dismay, the disgust for the false prognosis it is impossible to describe. No! let the outside world say what it may, candour in prognosis, founded on solid diagnosis, is the right course, both for patient and friends; it cannot kill a man stricken with organic disease; it cannot weaken a brave man, and it may give to one esteemed a coward resignation amounting to resolution, since death at some time is sure. For my own part, I should detest no man more thoroughly than that man who, seeing me lie hopelessly stricken at his feet, conveyed the idea by false word or look that my days or hours were not numbered.



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Dr Darwin

*Autotype of portrait (from
life of Erasmus Darwin by
Charles Darwin) after a picture
by J. Wright M.R.S. of Derby* *ARR.*

ERASMUS DARWIN, M.D. F.R.S. 1765-1846
DARWINIAN SCIENTIST

IN the early part of the eighteenth century at the fine old mansion called Harewell House, near to Aylesbury, a singular and learned man named Dr. John Lee. He was a Doctor of Laws, not of Physic, and he was well versed in both real and varied sciences. His work and leading part in scientific science, then in its infancy, he was a student of Egyptology, and he was the President of the Royal Astronomical Society. His wealth enabled him to give the best to science and scientific men; and from a year, when bright summers reigned, and the park and grounds of Harewell were most attractive, the excellent master of them would invite down to them twenty or thirty men engaged in scientific pursuits to spend not a day, but a fortnight, in the midst of good fellowship and congenial delights. I was so fortunate as to be more than once included in this charming circle, and many of my choicest after-friendships were made there. In the evening it was the custom for us to meet in the drawing-rooms and to discuss in a semi-formal manner some learned topic, ancient or current. One evening the subject brought before us was "Darwinism." The speaker was the well-known and much-esteemed Rev. J. B. Hensley, V.R.S., Vicar of



Dr. Darwin

*Received of the
Library of the
University of Cambridge
the sum of £100
on the 1st day of June 1871*

ERASMUS DARWIN, M.D., F.R.S., AND
DARWINIAN MEDICINE.

IN the early part of the fifties, there lived at the fine old mansion called Hartwell House, near to Aylesbury, a singular and learned man named Dr. John Lee. He was a Doctor of Laws, not of Physic, and he was also a man of wide and varied science. He took a leading part in meteorological science, then in its infancy; he was a student of Egyptology, and he was the President of the Royal Astronomical Society. His wealth enabled him to play the host to science and scientific men; and once a year, when bright summer reigned, and the park and mansion and grounds at Hartwell were most attractive, the excellent master of them would invite down to them twenty or thirty men engaged in scientific pursuits to spend not a day, but a fortnight, in the midst of good fellowship and congenial delights. I was so fortunate as to be more than once included in this charming circle, and many of my choicest after-friendships were made there. In the evening it was the custom for us to meet in the drawing-rooms and to discuss in a semi-formal manner some learned topic, ancient or current. One evening the subject brought before us was "Darwinism." The opener was the well-known and much-esteemed Rev. J. B. Reade, F.R.S., Vicar of

Stone, a kind of modern Hales, whose love for all science was boundless, and who was as well read in the past of science as in the present. To me the term "Darwinism" was new; but I soon picked up the fact that it referred to the theoretical teaching of the great Erasmus Darwin, one of the lights of the last century. In our debate this name became incidentally blended with that of Charles Darwin, now so entirely connected with the word "Darwinism." One of our party was the late Admiral Fitzroy, the distinguished meteorologist, who, as captain of the *Beagle*, had an intimate acquaintance with and claim to the close friendship of the later far-famed Charles Darwin the grandson of Erasmus. "I think," said the Admiral, "the elder Darwin will find a rival in his grandson, and that Darwinism, through Charles Darwin, may take a new and more enduring lease in the future." I made a note of the term and of the prediction relating to it.

Several years after that event I went to Leamington to deliver a lecture at the Philosophical Society of that town. I was invited to dine in the evening, after the delivery of the lecture, with one of the brightest and most genial representatives of physic I ever knew: the late Dr. Thomson. I was placed at table between Dr. Johnson, of Birmingham, and a lady whose name at the moment I was not conversant with. During the conversation that ensued, a portion of my lecture, referring to the famous Dr. Joseph Priestly, came under discussion. I observed that I had the pleasure of knowing personally Mrs. Joseph Parkes, the grandchild of Priestly, and that from her I had heard an account of the famous Lunar Club, which met on nights when the moon was in its fulness, and of which the great names of Priestly, Watt, Boulton, and Erasmus Darwin

appeared as the leading members. A smile of surprise passed between Dr. Johnson and my lady companion at this observation, as the former said, "If you would like to know more about the Lunar Club, Miss Galton, who sits by your side, will tell you much, for she is the grandchild of Erasmus Darwin and lives in this place with her mother, his daughter." As a matter of course, my inquiries went on more eagerly; and, to my great satisfaction, I received an invitation to breakfast next morning with the daughter of Darwin and Darwinism.

I found this daughter, Mrs. Galton, an aged lady, but full of vivacity and intelligence. Although breakfast was served early in order to allow me time to return to London by a morning train, Mrs. Galton was down ready to receive me, and was busy knitting. She met me with the most cheerful kindness, and during our repast related the particulars of one of the Lunar meetings at her father's house near Derby. The guests began to arrive about four o'clock in the afternoon; and she, who was for the day the young lady in charge of the household, received them. Priestly and Watt drove in, in a gig, from Birmingham; and others followed. Her father was in his garden, of which he was very proud, and in which he was always delighted to be. He carried a rose in his hand, and descanted on its beauty and the special characters of it, to the delight of his friends. After a short interval she made tea, to which all sat down, and during which friendly greetings and gossip, with many a timely joke, passed freely. Then the young maiden—she was nineteen at this time—was left alone whilst the philosophers retired into the library, or sanctum, to discuss the scientific problem that lay before them for consideration or solution.

The discussion over, the final event of the evening was the supper previous to the departure of the guests on their homeward journey, sometimes a distance of thirty to forty miles. But what mattered the distance to them under the light of the full moon? On the night in question Priestly and Watt once more occupied the gig, and started with an amusing disputation as to which of the two should take the reins, a dispute settled by the host in favour of the inventor of the iron horse.

If imagination might be let loose, one could well indulge it in speculation on the discourse entered into between Joseph Priestly and James Watt during their long ride. The subject they had been considering under the roof of Erasmus Darwin had been colour, and that they would have enough to debate upon in regard to it is not difficult to imagine. I will make no venture, but proceed to the illustrious subject of this biography, doing the best I can to read the man, and out of an immense pile of beautifully readable material, lying to left and right of me, extract the vital part as food for vital philosophy.

DETAILS OF LIFE.

I have not far to go for accurate details of the life history of Erasmus Darwin, for the place where I have to go is as sound as can be wished. In 1870 it was rumoured—I scarcely know from what source, some gossip of a literary friend of mine I believe—that I was preparing a life of the elder Darwin. The late Mr. Charles Darwin wrote me a note asking if it were the fact, offering me assistance if it were, and at the same time forwarding to me a recent volume on the "*Scientific Works of Erasmus Darwin, by Ernst Krause,*

translated from the German by W. S. Dallas, and with a Preliminary Notice by Charles Darwin himself." From this work I shall freely extract portions of the biographical facts required before turning to the works of the man himself as lessons for these columns.

Erasmus Darwin was born on December 12th, 1731, at Elston Hall, Notts. He was the fourth son of Robert Darwin, Esq., a member of a good old English family and possessor of a taste for scientific pursuits, a man also of stern temperance principles in respect to the use of alcoholic beverages. The father was bred for the Bar, but after he had married—and he seems to have espoused a learned lady—he gave up his profession, and retiring to Elston Hall, remained there to the end of his days. The brothers of Erasmus—whose Christian name, by the way, came from Erasmus Earle, Serjeant-at-law, whose daughter an earlier Darwin had married—would appear to have been fond of outdoor sports and exercises; but Erasmus Darwin had no such tastes. He was given to poetry and to the practical study of mechanics. He made an alarum for his watch, and invented an electrical machine of a rude kind, with which he would show little experiments. "He had always," his eldest brother informs us in a letter written to his nephew Robert, "a dislike to much exercise and rural diversions; and it was with great difficulty that we could ever persuade him to accompany us." At ten years old (1741) he went to Chesterfield School, and remained there nine years. At the age of sixteen, writing from school, which was apparently very comfortable for him, to his sister Susanna respecting the keeping of Lent, he shows himself off strongly in the matter of his temperance regimen: "I fancy you forgot in yours to inform me yt your Cheek was quite

settled by your Temperance ; but, however, I can easily suppose it. For ye temperate enjoy an ever-blooming health, free from all ye Infections and disorders luxurious mortals are subject to. The whimsical Tribe of Phisitians, cheated of their fees, may sit down in penury and Want ; they may curse mankind, and imprecate the Gods, and call down the parent of all Diseases, luxury, to infest Mankind, luxury more destructive than the Sharpest Famine ; though all the Distempers that ever Satan inflicted upon Job hover over ye intemperate, they would play harmless round our Heads, nor dare touch a single Hair. We should not meet those rare thin and haggard countenances which every day present themselves to us. No doubt men would still live their Hunderd, and Methusalem would lose his Character. Fever banished from our Streets, limping Gout would fly ye land, and Sedentary Stone would vanish into oblivion, and death himself be slain." The writer adds that he could "for ever rail against luxury and panegyrisse upon abstinence," but excusing himself, writes a few more lines which are not legible, and prays "hast, supper being called, and he very hungry."

In 1750, in company with his two elder brothers, he went to St. John's, Cambridge, where the means of the brothers were so small, they were obliged to mend their own clothes. Erasmus won the Exeter Scholarship, worth £16 per annum. In 1754 he took Bachelor of Arts at the head of the Junior Optimes, and he began the study of medicine, courting at the same time the Muses. Proceeding to London, he attended a term of lectures at the school of William Hunter ; thence, in the autumn of 1754, he moved to Edinburgh, where he studied patiently, and under such strict economy that from July 13th to October 13th his board cost him

£6 12s. In Edinburgh at this time the school of Boerhaave was dominant, a school of medicine as dogmatically chemical and humoral as the narrow school of the present hour. Then, as now, the external influences of nature, or rather the influences of external nature on the human body, were ignored in the schools; and the nervous system was looked over as if it existed without uses. Darwin, young as he was, had sense enough to be sceptical, and setting the narrow hypotheses at their true value, became in his more matured life a leader in the broader view which accepted the influence of external vibrations, acting through the senses, as the most potent of all forces governing the organism in its current course, and storing up energy for future application and demonstration of that power which, for want of a better word, we call vital. In 1754 his father, whom he describes as "frugal, but not covetous," a man tender to his children, but who kept them at an awful distance, died, and next year he returned to Cambridge to take his degree as Bachelor of Medicine. Afterwards he went back to Edinburgh for further study; and, his studies complete, he settled himself down in Nottingham to medical practice. Practice did not come, and so, after waiting for two or three months, he moved to Lichfield. He was not idle at Nottingham, although practice failed him, for whilst there he wrote letters in English and in Latin to young Reimarus, son of a famous German philosopher, to whom also he dedicated a poem. He corresponded with Gurney on the subject of shorthand, acquiring that useful art for himself and turning it to practical account, and he got into a little difficulty about writing an anonymous letter to a surgeon who, as he fancied, had taken a fee for

performing an operation on a poor man who was unable to pay the sum demanded. Two-thirds of the fee were returned, and Darwin himself made up the rest; but the first part of the act was Quixotic and not so discreet as it might have been, an enthusiasm of youth which was afterwards regretted.

It was in the November of 1756 that Erasmus Darwin opened his career in the city of Lichfield; and in that city he remained until 1781, when he moved to Derby. In Lichfield practice came; and from there his name began to extend abroad, carrying with it the fame of industry, of original power, and of advanced scholarship, both in medicine and science. A little more than a year after settling in his new sphere he married a charming young lady, in her eighteenth year, Miss Mary Howard, with whom he lived in perfect conjugal happiness up to her death in the year 1770, and by whom he had three children: Charles, born September 3rd, 1756; Erasmus, born in 1759; and Robert Waring, father of the Charles Darwin of our time, born in 1766.

The professional profits of Darwin rose in seventeen years from £18 7s. 6d. to £1,625 3s. Eighteen years later still he calculated, for the income tax return, that he was making £1,000 a year, from which he deducted £200 for a liveried servant, four horses, and a day labourer, and £200 for travelling expenses and chaise hire. He remained a widower for eleven years after the death of his first wife, and then married the widow of Colonel Chandos Pole, a marriage of ardent and continued affection. Soon after this second marriage, in 1781, he moved to Radburn Hall, and two years later to Derby, where his prestige was maintained up to the last. Lichfield and Derby therefore may alike claim him as their most celebrated Æsculapian.

THE WORKS OF ERASMUS DARWIN.

Erasmus Darwin has been considered a prolific writer, but it is worthy of note that his authorship of any importance commenced when he was advanced in life. His poem, "*The Loves of the Plants*," was not prepared for press until 1788—that is to say, when he was in his fifty-ninth year; his other works were continued to 1800, two years before his death, and one was posthumous. Four particular treatises comprise these works; a first, entitled "*The Botanic Garden*," including a poem on "*The Loves of the Plants*" (1788); a second, "*The Zoonamia; or The Laws of Organic Life*" (1794); a third, "*Phytologia; or The Philosophy of Agriculture and Gardening*" (1800); and a fourth, "*The Temple of Nature*," a poem, posthumous. In addition there should be noted a short treatise entitled "*A Plan for the Conduct of Female Education in Boarding-schools*" (1797).

The works themselves are not numerous, but they are, with the exception of the last, large in themselves as books, and still larger and more comprehensive as philosophies. They exhibit their author in so many capacities, in so many qualities, he appears as a body of many men rather than as one. He is a poet, a natural philosopher, a physician, an agriculturist, a philanthropist, a scholar, and withal—for this must not be omitted—a man of the world. I shall prefer, in treating of his works, to consider them as springing from these different sources in the same individual rather than notice the different works one after another. This will be a readier method of understanding the author, because his labours, although, by accident or by circumstance, divided into books, are

really indivisible as teachings. They all refer to some few choice subjects that were on his mind, and certain of them were clearly conceived and ready for publication before others were actually published. "*The Zoonamia*," for example, was ready, or nearly so, for the press in 1775, and was intended to be posthumous. It was published twenty-three years later, after "*The Botanic Garden*," which probably was not commenced when "*The Zoonamia*" was completed.

THE POET.

From his earliest days Darwin considered himself a poet, although he published nothing important of the poetic kind until late in life. He devoted himself for twenty or more years, so he has related, to the practice of his profession; and in sending a poem of little pretence to a friend, affirmed that he would not again tempt the Muses. He did not keep his word in this respect, as we now know. Buying about eight acres of land near to Lichfield, he made a botanic garden there, and that seemed to inspire him with the idea of the poem which has placed him high up on the roll of medical poets. Amongst the poets of physic he holds a leading place. He does not approach Virgil, if Virgil may be claimed as belonging to us; he would scarcely rank with Akenside; he would not rank with Goldsmith or Keats; but he is far above Blackmore, with whom he is sometimes placed, and is perhaps more than equal to Armstrong, who in mode is much the same. Immediately after the appearance of "*The Botanic Garden*" his poetry was much esteemed, and, by some competent judges, was thought to be fine and even immortal. Tastes after a while changed. The poem called "*The Loves of the Plants*" was parodied under the title

"The Loves of the Triangles," and in course of time the whole of the poetry was stigmatised as heavy, cumbersome, mechanical, and, with the exception of a few passages, as mere lines strung together in meaningless order; didactic art in chains. The true view to take on the matter is to consider Darwin truly as a didactic teacher, writing poetry, without spontaneity, for didactic purposes. This was an error. When he was teaching in prose, he was effective, natural, perspicuous; when he passed to poetry, he became so involved in imagery, so obscure, so spell-bound, he was obliged to add the most copious notes in prose, in order to explain to his readers what he had been driving at in poetry. Perchance a good many poets had done well by following the same method, but these would not be poets of the highest class. Poets of the highest class make their song tell its own story, and, whether they appeal to the head or the heart, let every effort carry its own and perfect meaning. It has been urged, as if in excuse for Darwin, that he was surpassed by Wordsworth and the later school of poets because they appealed to the emotions instead of the intellect, and that from them poetry has taken a new lease of life, a life altogether apart from the poetic lectures which Darwin delivered. This is like saying that didactic poetry was doomed and went out with Darwin. The hypothesis cannot be admitted for a moment. Burns was a teacher; his "Cotter's Saturday Night" in a simpler sphere is as didactic as Darwin's account of the universe. No, that was not the fault of Darwin. His failure, in so far as his poetry was a failure, lay in three directions. He tried to expound by poetry subjects so majestic, so near to the ways and works of God Himself, that the grandest poetic

genius who ever lived could not have winged his way to a perfect record by poetic art. He was daring heights which Dante himself could not have scaled. Next, in what he did he was not spontaneous, or was spontaneous at moments very short and light. Lastly, he chose the most inflexible, cumbrous, and dismal form of verse, verse that did not suit his own genius, verse that did not differ from the everlasting grind of a saw, that had to be persistently reset, in order that it might make its way with ease and possibility. In reading his poems one can almost see the author at his labours, wearying, falling off tired, returning, reworking, and feeling at last that the student must have the aid of a crib in prose ere ever he can understand the nature of the description. We turn to Canto I., "The Economy of Vegetation," and drop at once on a few lines demonstrative of the peculiarities named above. The Goddess of Botany addresses the Nymphs of Fire:—

"Nymphs! you disjoin, unite, condense, expand,
And give new wonders to the chemist's hand;
On tepid clouds of rising steam aspire,
Or fix in sulphur all its solid fire;
With boundless spring elastic airs unfold,
Or fill the fine vacuities of gold;
With sudden flash vitrescent sparks reveal
By fierce collision with the flint and steel,
Or mark with shining letters Kunkel's name
In the pale phosphor's self-consuming flame.
So the chaste heart of some enchanted maid
Shines with insidious light, by love betrayed;
Round her pale bosom plays the young desire,
And slow she wastes by self-consuming fire."

To the common reader this poetic description is meaningless; in order, therefore, to make it plain, four and five hundred words, in prose, are notes, with an additional note to explain

that Kunkel was one of the discoverers of phosphorus. The notes themselves are most instructive quite apart and away from the poem, and only serve to kill all the romance about the chaste heart of the enchanted maid who comes upon us with such dazzling surprise and disappears in so awful a consummation.

It is not always, however, that the author of "*The Botanic Garden*" surfeits his readers with lumbering verse, or puzzles them with poetic philosophies requiring prose notes for their elucidation. When he becomes poetical, in the true sense of the term, he gives, as Charles Darwin says he did in some love pieces to his second wife, bits of real beauty. Here, for instance, is a little song to May, which, being free of philosophy and full of sweetness, rings out like music from previous noise:—

"Born in yon blaze of orient sky,
Sweet May! thy radiant form unfold,
Unclose thy blue voluptuous eye,
And wave thy shadowy locks of gold.

"For thee the fragrant zephyrs blow,
For thee descends the sunny shower;
The rills in softer murmurs flow,
And brighter blossoms gem the bower.

"Light Graces, dressed in flowery wreaths,
And tiptoe Joys their hands combine;
And Love his sweet contagion breathes,
And laughing dances round thy shrine.

"Warm with new life, the glittering throngs
On quivering fin and rustling wing,
Delighted, join their votive songs,
And hail thee, goddess of the spring."

The two specimens of Darwinian poetry rendered above must suffice here, but they are poor representations of "*The Botanic Garden*." "*The Botanic*

Garden” lives, and will ever live, on its notes, notes not of musical song, but of scientific prose. It would be difficult to find in any author so much experimental description, so much originality of thought, as may be found in the explanatory note passages, where classical joins with modern learning, and the future is anticipated with such foresight that the mind of the present day is positively startled as it receives the early impulses of what is now pure matter of fact and common realisation.

THE NATURALIST.

The natural philosophy of Darwin is included in the didactic work “*The Botanic Garden*,” and it will be observed that, whereas he wrote “*The Zoonamia*” first, he published it after the appearance of “*The Garden*.” We may infer pretty clearly from this that he desired to ensure the publication of his most ambitious claim to immortality whilst his mortal powers were in their hale, if not their pristine, vigour. He had thought long over the propositions he wished to announce; and so startling were they, he veiled them in a kind of poetical allegory, as if it were pardonable in a poet to say what he liked, even on matters of solid philosophy, without giving offence and without being charged either with madness or heresy. His introduction is an apology. “It may be proper,” he observes, “to apologise for many of the subsequent conjectures on some articles of natural philosophy, as not being supported by accurate observations or conclusive experiments. Extravagant theories, however, on those parts of philosophy where our knowledge is yet imperfect, are not without their use, as they encourage the execution of laborious

experiments, or the investigation of ingenious deductions, to confirm or refute them."

The mind of Erasmus Darwin was from the first free as well as original, imaginative as well as mechanical, nervous as well as intrepid, cautious as well as bold, modest as well as ambitious, sceptical as well as conclusive. His was, in every way, a strange mind, a mind not merely dual, but strongly tripartite. In him passion glowed from the organic centres; imagination, powerful and aggressive, literally vaulted into being; while reason towered as a reigning power. In him emotion, imagination, reason, knowledge, wisdom, held equal sway. Hence what seems so complex in his labours, so hard and so soft, so simple and so difficult, so mystical and so clear, is always before us in his writings. He knew the value of experiment, and conceived experimental data which he had never practised, never seen. He was therefore believed in and disbelieved in with about the same measure of credence; for those who believed in him could not prove, and those who disbelieved could not refute, by any decisive procedure. So he remained a mystery until his grandson, Charles Darwin, playing Aristotle to his Plato, came to shed steadier light from patient and laboured experiment, and to illuminate his foreshadowings and shrewd guesses of natural workings and works by the magic of industry.

His strange genius shines out effulgently in the observations he makes on the development of living things. He had been bred up in the school which Sterne so whimsically describes in the opening chapter of "*Tristram Shandy*." The homunculus was the primary man, and the primary analogue of the human animal was in that completed being, *in petto*.

Ingenious philosophers, Darwin explains, have found no difficulty in conceiving the manner of reproduction of animals; they have supposed all the numerous progeny to have existed in miniature in the animal originally created, and that these infinitely minute forms are only evolved or distended as the embryo increases in the womb. These, the embryos, must possess a much greater degree of minuteness than that which was ascribed to the devils that tempted St. Anthony, of whom "twenty thousand were said to be able to dance a saraband on the point of the finest needle without incommoding each other."

It was against this theory that Erasmus Darwin raised his objection, and in displacement of which he introduced the theory of evolution. Krause has ably epitomised Darwin's views on this subject under four heads, observing that in them, fifteen years before the appearance of Lamarck's "*Philosophie Zoologique*," the principles of evolution were completely set forth. Under the first of these heads the author of "*The Botanic Garden*" says:—

"When we revolve in our minds first the great changes which we see naturally produced in animals after their nativity, as in the production of the butterfly with painted wings from the crawling caterpillar, or of the respiring frog from the subnatant tadpole, from the feminine boy to the bearded man; when we think over the great changes introduced into various animals by artificial cultivation, as in horses which we have exercised for the different purposes of strength or swiftness, or in dogs which have been cultivated for strength and courage, as the bulldog, or, for acuteness of their sense of smell, as the hound and spaniel, or, for the swiftness of his foot, as

the greyhound, . . . and add to these the great changes of shape and colour which we daily see produced in smaller animals from our domestication of them, or from the difference of climates and even of seasons; when we enumerate the great changes produced in the species of animals before their nativity and continued to their posterity, . . . and become convinced that the fœtus or embryo is formed by the apposition of new parts, and not by the distension of a primordial nest of germs included one within another like the caps of a conjurer; when we revolve in our minds the great similarity of structure which obtains in all the warm-blooded animals,—then one is led to conclude that ‘they have alike been produced from a similar living filament: that in some this filament, in its advance to maturity, has acquired hands and fingers, with a fine sense of touch, as in mankind; in others it has acquired claws or talons, in others toes with an inturning web or membranes, in others cloven hoofs; while in the bird kind this original living filament has put forth wings instead of arms or legs, and feathers instead of hair’: and that from their fist rudiment, or primordium, to the termination of their lives, all animals undergo perpetual transformations, which are in part produced by their own exertions in consequence of their desires and aversions, of their pleasures and pains, or of irritations, or of associations: and that many of these acquired forms or propensities are transmitted to their posterity.”

In many other lines of verse and prose Erasmus Darwin evolves the theory of evolution. He argues that the three great objects of desire—lust, hunger, and security—have changed the form of many animals by their exertions to gratify them; and that a great want

of one part of the animal world has consisted in the desire of the exclusive possession of the females, another in the procuring of food, "which has diversified the forms of all species of animals," while the third, security, has diversified the forms of the bodies and the colour of them. On the other hand, some animals have developed means of pursuit and destruction, "all which seem to have been formed by the original living filaments, excited into action by the necessities of the creatures which possess them, and on which their existence depends."

In summary of his great subject Darwin adds:—

"From thus meditating on the great similarity of the structures of the warm-blooded animals, and at the same time on the great changes they undergo both before and after their nativity, and by considering in how minute a portion of time many of the changes of animals have been produced, would it be too bold to imagine that in the great length of time since the earth began to exist, perhaps millions of ages before the commencement of the history of mankind—would it be too bold to imagine that all warm-blooded animals have arisen from one living filament, which THE GREAT FIRST CAUSE endued with animality, with the power of acquiring new parts, attended with new propensities, directed by irritations, sensations, volitions, and associations, and thus possessing the faculty of continuing to improve by its own inherent activity and of delivering down those improvements by generation to its posterity world without end?"

It is with real regret I leave Erasmus Darwin as the philosopher and naturalist, there is so much to be said of him in those characters. He is an Atlas in the world of natural history. The world of modern natural

history rests on his broad shoulders. His was the mind that gave the original thought of evolution. It is not a mind that in this day of mechanism is likely to be much appreciated. His thoughts have been the energy for what is now considered to be the greatest form of work, the deep analysis of natural details. In brief, as the originator of first principles Erasmus Darwin is still Darwin, and Darwinism is Erasmus. Say that he blended poetry with prose, imagination with fact, emotion with reason; say that his animal propensities were as strong as his intellectual—and perhaps they were—the admission is all the same in favour of Darwinian greatness. He had strong passion. Yes. But if he had wanted passion, there had been no Darwinian doctrine; the passion was the impulse springing from and out of him: the residue was the glorious treasure, the gold left in what had been his living, burning, fiery furnace. What prudes would call his vices were inseparable from his virtues; they were part of his stock of vitality implanted by Nature, from her exuberant hand.

THE PHYSICIAN.

The poet Darwin, the philosopher Darwin, was of all things a physician. He was exceptional in this respect. He was one of the rare men of physic who, being a philosopher, could be esteemed as a practitioner. As a rule, any man of common industry and common honesty can gain the so-called highest places in medicine without being removed one step from the most commonplace knowledge of the art. The art without the science is all-sufficient and far better for worldly sneers than much science. It must be so, because in current medicine there is no bar of judgment.

Nature always runs ahead of the doctor for good or for evil, and if he do but content himself in watching her course, if he do not interrupt her, if he be shrewd enough to calculate and foretell her proclivities without giving her credit, and if he be skilful enough to take credit for all her successes, he must get on. In the practice of the art, therefore, genius has no abiding-place, but is usually expunged from its place ruthlessly. Erasmus Darwin, holding what were considered by most persons even extravagant views on philosophical subjects, posing as a poet, and writing with almost a contempt for public opinion, was nevertheless appreciated as so able a physician that his fame reached London, reached the Court, and led to all but an invitation to him to come to the metropolis as a Court physician; it reached also the profession itself with such favour that a leading physician of London, Dr. Warren, went incognito to Derby to consult him as "the greatest living medical authority."

There are reasons for this anomalous position, however, which go a long way in explaining it. Darwin made his medical reputation first, and let his originally conceived science come as the crowning work of his life. He, to use a stock phrase, "stuck to practice." He was considered by the common folk, who surrounded him most nearly, as an oddity, a sort of wizard; but that mattered little: they, if anything, trusted him the more as a mystery; his immediate friends thought him a man of genius, who was bound to a profession by circumstances, and who let nothing divert him from the regular course of physic. He also evidently knew his trade as well as his art; and concealing the art in its higher manifestations, kept the trade well in hand. These were good contributories to success, and into

them all there ran one other power: passivity in professional and scientific action. Living at Lichfield and at Derby, Erasmus Darwin, monarch of physic in his own limited and quiet sphere, crossed no rival who could assail him or his ways. So little did he commingle with the troubles of metropolitan life he did not so much as sign the admission book of the Fellows of the Royal Society, and probably never read a lecture or delivered an address in London. He did not, therefore, come into personal collision with men who would have claimed to have been his peers, but in the calm, sequestered vale of life in which his course was cast passed safely into the haven of the illustrious dead.

No cunning pettifoggers crossed his way;
No jealous copyists his steps waylaid;
Unwatched, untrammelled, he could dare to stray
Wherever genius lent its guiding ray,
Till death, his only foe, the final visit paid.

His grand contribution to medicine was his "*Zoönamia; or, The Laws of Organic Life*," a work published in two quarto volumes, bearing date May Day, 1794, and dedicated "To the Candid and Ingenious Members of the College of Physicians, of the Royal Philosophical Society, of the Two Universities, and to all those who study the Operations of the Mind as a Science or who practise Medicine as a Profession." "The purport of the work," so he tells us, "is to reduce the facts belonging to animal life into classes, orders, genera, and species, and, by comparing them with each other, to unravel the theory of diseases." He subordinates the great medical theories current in his day respecting animal hydraulics and chemical changes as the essences of life to the idea that "animation is

the essential characteristic " of vitality ; and although he does not ignore chemical and the mechanical views—nay, indeed, uses them both freely—he uses them only as if they were servants, their true and proper place. A hundred years less one have passed since "*Zoonamia*" appeared, and yet "*Zoonamia* " remains as fresh as ever, as needful as ever. We working men of the present century have lived all through the past on the inventive genius of the last century, adding possibly not one new thought, but working out details from that which has been bequeathed to us by our predecessors, who sowed the seed of our harvest.

In "*The Zoonamia* " it is assumed that in the whole of nature there are two essences or substances, one of which may be termed spirit, the other matter. The spirit possesses the power to commence or produce motion, the matter to receive and communicate it ; so that motion considered as a cause immediately precedes every effect ; and considered as an effect, it immediately succeeds every cause. The motions of matter are of two kinds : the primary and the secondary. The primary are divided into three classes : those of gravitation, of chemistry, and of life ; the secondary are those which are given to, or are received from, other matter in motion, and are distinguished by the circumstance that the velocity multiplied into the quantity of matter of the body acted upon is equal to the velocity multiplied into the quantity of matter of the acting body. Darwin deals with the motions of the third class : those of life, including all the motions of the animal and vegetable world, as well as those of the vessels which circulate their juices, and of the muscles which perform their locomotion, and " of the organs of sense which constitute their ideas."

In section iv. of vol. i. of "*The Zoonamia*" he formulates what he considers to be the laws of animal causation. He argues that the fibres which constitute the muscles and organs of sense possess a power of contraction, and the circumstances attending the exertion of that power constitute the laws of animal motion, as the circumstances attending the exertion of the power of attraction constitute the laws of motion of inanimate matter. The spirit of imagination is the immediate cause of the contraction of the animal fibres; this spirit resides in the brain and the nerves, and is liable to general or partial diminution or accumulation. But the stimulus of bodies external to the moving organ is the remote cause of the original contractions of animal fibres. A certain quantity of stimulus produces irritation, which is an exertion of the spirit of animation exciting the fibres into contraction. A certain quantity of contraction of animal fibres, if it be perceived at all, produces pleasure; a greater or less quantity of contraction, if it be perceived at all, produces pain; these constitute sensation; a certain quantity of sensation produces desire or aversion; and these constitute volition. All animal motions which have occurred at the same time, or in immediate succession, become so connected that when one of them is reproduced the other has a tendency to accompany or succeed it. When fibrous contractions succeed or accompany other fibrous contractions, the connection is associative; when fibrous contractions succeed sensorial motions, the connection is causative; when fibrous and sensorial motions reciprocally introduce each other, it is termed catenation of animal motions. All these connections are produced by habit—that is, by frequent repetition. The sensorium possesses four

faculties—namely, irritability, sensibility, voluntariness, and associability—and these in their active state mean irritation, sensation, volition, and association. *Irritation* is an exertion of some extreme part of the sensorium residing in the muscles or organs of sense, in consequence of the appulses of external bodies. *Sensation* is an exertion of the central parts of the sensorium, or of the whole of it, beginning at some of those extreme parts which reside in the muscles or organs of sense. *Volition* is an exertion of the central parts of the sensorium, or the whole of it, terminating in the muscles or organs of sense. *Association* is an exertion of some extreme part of the sensorium residing in the muscles or organs of sense in consequence of some antecedent or attendant fibrous contractions.

On the principles above stated, the method of the study of the phenomena of disease is based on the Darwinian system. It sounds strange to us now, but when it is followed out it is far more methodical and philosophic than any of the mere mechanical speculations and inventions of words which characterise our present school. That the theory blended well with practice is indicated throughout the whole of the work, and is particularly conspicuous in matters relating to treatment. Some points are of such singular interest in these respects, I must refer to them.

“Matter produced by suppuration (section xxviii. 2) will lie concealed in the body many weeks or even months without producing hectic fever; but as soon as the wound is opened so as to admit air a hectic fever supervenes even in a very few hours, which is probably owing to the azotic part of the atmosphere rather than to the oxygen.

“When wounds are to be healed by the first

intention, as it is called, it is necessary carefully to exclude the air from them. Hence we have one cause which prevents pulmonary ulcers from healing, which is their being perpetually exposed to the air."

He has a very curious and clever notion respecting what he calls "retrograde absorption." He shows that some fluids may find their way from the intestinal canal into the bladder by another passage to the bladder besides the long course of the arterial circulation. The way is that which Hewson demonstrated when he showed that the intestinal absorbents are joined with the urinary lymphatics by frequent anastomosis; "and as there is no other road, we may justly conclude that these fluids pass into the bladder by the urinary branch of the lymphatics, which has its motions inverted during the diseased state."

On the subject of diabetes Darwin is remarkably clear and advanced, advanced, I had almost said, for the present day. He records some good cases of the disease, illustrating the value of opium as a curative remedy, and offering sound advice on the subject of food and feeding. But the most important remarks which he makes relate to the cause of the leading symptom. He believes that the saccharine matter with which the urine of diabetic patients so abounds does not enter the blood-vessels like nitre and asparagus; but the process of digestion resembles the process of the germination of vegetables or of making barley into malt, as the vast quantity of sugar found in the urine must be made from the food taken and the drink. Secondly, as the serum of the blood is not sweet, the chyle appears to have been conveyed to the bladder without entering the circulation of the blood, since so large a quantity of sugar as was found in the urine

in one case—namely, twenty ounces a day—could not previously have existed in the blood without being perceptible to the taste. The mode of the excretion in this instance he supposes to be by retrograde absorption; and he suggests that if the mesenteric glands were nicely inspected in those who die from diabetes, and if the thoracic duct and the larger branches of the lacteals and of the lymphatics which arise from the bladder were well examined by injection or by the knife, the cause of diabetes might be more certainly understood.

The chapter on “Diseases of Irritation” is rich in thought of an original kind. Darwin evidently knew for himself Bacon’s axiom, “Heat and cold are the two hands of nature,” for his descriptions of the effects of heat and cold on the living body are truly master-pieces. So also is the section (xxxvi.) on the periods of diseases; I know no work which is so careful and so detailed on the subject of periodic manifestations of particular symptoms attending special diseases as this chapter. But indeed all the first volume is filled with matter that excites the warmest admiration, matter that will not die, however long it may be concealed.

The second volume of “*Zoonamia*” is nosological, and may, perhaps, be considered as an attempt to force into one line of practical thought the ingenious theories of the author. The classification is admittedly too mechanical, but there are comprehensive sections which redeem all errors of the kind named. The essay on the “Sympathetic Theory of Fever” is rich in suggestion, rich equally in description of phenomena, and especially in the description of what he calls the “*transports*” of the different systems of organs, respiratory, and so on, during the febrile state. He

gave this outline of the sympathetic theory of fevers to distinguish it from the mechanical theory of Boerhaave, the spasmodic of Hoffman and Cullen, the putrid of Pringle, and to constitute a foundation and scaffolding for a firm and well-constructed building, "which may stand unimpaired, like the Newtonian philosophy, a rock amid the waste of ages."

A VERSATILE SCHOLAR.

There are many other characteristics in the life of Erasmus Darwin to which I ought to refer. Indeed, in no life sketch I have written for ASCLEPIAD have I felt so much regret at leaving so much unsaid that ought to be said. I have stated that, in addition to being a poet, a natural philosopher, and a physician, he was an agriculturist, a philanthropist, a scholar, and a man of the world. On these last four parts of his versatile nature I must not long dwell. "*The Phytologia; or, The Philosophy of Agriculture and Gardening*," is a volume as remarkable as "*The Zoonamia*." In "*The Phytologia*" he appears as a sanitarian of the first order, laying down in other but almost in as precise words Mr. Ward's sanitary proverb, "The sewage to the land; the rain to the river." He advocated the necessity of free ventilation of the house, taught the grand rule of universal cleanliness, and in many other particulars anticipated many lessons which we sanitarians of the present day have urgently enforced. In another and separate treatise, entitled "*A Plan for the Conduct of Female Education in Boarding-schools*," he contrasted favourably emulation as against punishment, and insisted that girls should be taught such exercises as skating, swimming, and funambulation, or, dancing on the straight rope.

We have seen that in his earliest days he was mechanically inclined, and all through his life he followed the same bent. He invented a new kind of carriage; a carriage wheel with elastic spokes; a phonetic alphabet; a speaking machine; and a speaking tube, which, to the marvel of his rustic neighbours and the alarm of one of them, he used in his own house.

To Erasmus Darwin as a philanthropist we may give credit that he was one of the earliest physicians to recommend the humane treatment of the insane; and bearing on his physiological insight, Charles Darwin relates with some natural pride that Rosenthal's experiment described by Dr. Lauder Brunton, showing that when an animal is exposed to a rather high temperature the cutaneous vessels become paralysed by the heat and remain dilated even after cold has been applied, leaving the blood exposed over a large surface and causing it rapidly to cool, was anticipated by his grandfather in "*Zoonamia*," vol. ii. p. 570, as a mode of accounting for colds and catarrhs.

IN EPITOME.

In epitome, Erasmus Darwin was the most astounding medical light of the eighteenth century, and will probably live longer than any Æsculapian of that century; may, as long as any of any century. He was a man of impulse as well as of sustained energy. His mind was born free; and no fear of superstition, no fear even of death, assailed him. He overcame all difficulties by his perseverance. Impeded in speech by stammering, he succeeded, as cleverly as his friend Priestly, in overcoming that defect; and how boldly and firmly he wrote a name is shown in the autograph kindly lent me by daughter, Miss Galton, to add to the portrait

of the present ASCLEPIAD, an autograph which he had put on a visiting card embellished with a tasteful mounting. That he left strength in progeny goes without saying; Charles Darwin, Major Leonard Darwin, now M.P. for Lichfield, and Mr. Francis Galton, are sufficient proofs of this fact.

Erasmus Darwin lived to reach his seventy-first year, dying at Breadsall Priory on April 18th, 1802. He did not expect to live long, but he had recovered from a short illness, and the night preceding his death was as cheerful as usual. At 7 a.m. on April 18th he was seized with rigor, went to the kitchen to warm himself, returned to his study, laid himself on the sofa, became faint and cold, was moved into an arm-chair, and expired, without pain, before 9 o'clock.

One point in this intense life I reserve to the end, that no one may be prejudiced. Erasmus Darwin was one of the earliest, one of the staunchest, of Temperance Reformers. Mr. Charles Darwin, writing to me as President of a Temperance banquet, held at the Crystal Palace on July 11th, 1876, expressed himself as follows:—

“I have been brought to the conviction from the very large experience of my father and my grandfather, which has extended over a century, that no cause has led to so much suffering and inherited ill-health as the consumption of alcohol.”

The works of this illustrious grandsire of the illustrious author of “*The Origin of Species*” are replete with the same truth, a truth that can never be confirmed enough, though doubts should ever sleep.

A THEORY OF A NERVOUS ATMOSPHERE.

PART I.

THE study of the functions of the nervous systems of animals has, since the days of the illustrious Willis, been the most fascinating of physiological pursuits, and, at the same time, the most deceptive. Willis, if I read him correctly, grasped in his primitive and wonderful researches more of the truth and more of the difficulty of the subject than any other single observer who has followed him; and although, in the two hundred years that have elapsed since his time, thousands of experimental inquiries have been instituted, and, I had almost said, thousands of hypotheses have been framed and given forth, work remains before us of the present day which, except by the aid of some grand generalisation founded on facts of observation, can in no way be expected to be accomplished in our generation. For this important truth requires to be laid bare whenever we touch on the subject of nervous action: that every hour not only brings something new to be learned, but something old also to be questioned or relearned which we have thought wholly accepted or discarded. Take, for example, the one curious question respecting the motive character of the nervous mechanism: the current or motion that passes, or is assumed to pass,

along the fibres of nerve from the centres towards the periphery, and from the periphery towards the centres. What is this? Is it an imponderable agency, or is it a refined fluid with which the nerves are charged, a fluid susceptible of motion by any and every force—by heat, by mere mechanical impulse, by electrical excitation, by light—or is it derived from one force which the nerve simply and purely conducts, as the metallic wire conducts the electrical charge? On these points, it seems to me, we stand in much the same position as the older physiologists stood when Haller endeavoured to reason upon the rate at which an impression travels from the brain to the muscles, and supplied the sentence—"Ita invenio, summam tamen celeritatem esse muscularis liquidi, ut non minus quam 9,000 pedes ni minuto primo percurrat."* I mean that we wait to know how the motion is transmitted, and what part the nerves play, whether they lay up force, produce it, convey it, or perform all these duties.

MUNRO ON A NERVOUS FLUID.

The original idea respecting nervous function is well described by the anatomist Alexander Munro. Most authors, he tells us, up to the time when he wrote (1783), supposed that the nerves are tubes or ducts conveying a fluid secreted in the brain, the cerebellum, and spinal marrow. But, he adds, of late years, several ingenious physiologists have contended that a secreted fluid would be too inert for serving the offices performed by the nerves, and therefore have supposed that the nerves conduct a fluid the same as, or similar to, the electrical fluid. These philosophers reason, he continues, that two arguments stand on their side: one,

* Haller, *Elementa Physiologiae Corporis Humani*, L. x.

the rapidity of motion through the nerves ; and the other, that some animals, as the torpedo and the gymnotus electricus, have the power of giving an electrical shock, and that, on dissecting these animals, a piece of machinery proper to them is discovered in which large and numerous nerves terminate. Thence he (Munro) undertakes to refute these arguments. Of the first, the reasoning on the question of velocity, he urges that, if the nerves are constantly filled or charged with fluid (which, from our constantly perceiving injury done to even the most remote part, we have reason, he thinks, to suppose must be the case), an impulse given to that fluid at the brain may be suddenly communicated to the most distant organ, although the velocity of the fluid be very small. Nay, in fact, we find, after cutting through the nerve of a muscle, that, by irritating the nerve, repeated motions can be performed ; whereas, by Dr. Haller's theory of great velocity from end to end of the nerve, as the nerve now wants supply from the reservoir in the brain, the fluid should be exhausted by a single effort of the muscle. Such argument as this no more proves, he contends, that the nervous fluid travels with great velocity, than our letting out, in the space of a minute, a hundred successive drops of water from the end of a pipe a mile distant from the reservoir which filled it would prove that the water in that pipe was moving at the rate of a hundred miles a minute.

And, as to the argument on the torpedo and gymnotus electricus, he urges that all we can conclude from the facts is that the nerves enable the animal to perform its proper office of collecting the electrical fluid, but without directly furnishing to it any of that fluid, just as we, by rubbing a glass tube, excite electricity without there being any reason to suspect that the electrical

matter is in particular derived from the nerves of our hand, since the electricity could be as readily excited by the hand of a dead man as by that of a living rubbed against the glass with the same force.

Thus Munro in his day endeavours to refute the hypothesis of electrical current in nerve, and of it as the excitant of natural muscular motion; and to what he has said already he puts forward other and cogent arguments. When he cuts a nerve across and brings its parts again into contiguity, he does not, he maintains, restore the offices of the nerve immediately. Nay, when he divides a nerve in a frog, and lets the divided parts grow together again, the influence of the nerve beyond the incision is, generally, not restored. On the other hand, the well-known effect of compression on the nerves of a sound animal and the experiment of producing repeated contractions of muscle by pressing on a nerve after cutting it across indicate that the energy depends on matter *capable of being affected by simple pressure*.

The hypothesis is simple, but it is not, admits the anatomist, proved. Still to say that the offices of the nerves are not performed by a secreted fluid merely because we cannot comprehend how any part of the blood, or any humour prepared from it, could render the mind sensible of an injury or throw a muscle into action, is saying a great deal too much, for in the generation of animals effects more incomprehensible and astonishing seem to depend on the secretion and mixture of the fluids of the testes and ovaria, the brain, the nerves, the nervous energy, and the complex fabrics of other organs being thereby produced.*

* *Observations on the Structure and Functions of the Nervous System.*
By Alexander Munro, M.D. Edinburgh: 1783.

As it is refreshing oftentimes to listen to the simple and natural expressions of children on things in nature which they are trying to comprehend, so is it refreshing oftentimes to read, in philosophy, the utterances of those who first began to study Nature in the way in which she opens herself to the first searchers after her secrets, and, I confess me, the reading of Munro, which I have ventured to revive, conveys to my mind a charming freshness of thought, and a suggestive lesson much to be valued. When, again, I turn to another chapter of his book, on the question "Whether the Nerves convey Nourishment to our Organs," I find a new freshness of discussion replete with instruction. But that which I would dwell upon at this present moment is the fact that even yet we are uncertain whether the original theory of a subtle or refined fluid charging the nervous matter, and *capable of being affected by simple pressure*, be not correct after all, and whether the study of the phenomena of electrical manifestations which Galvani mainly instituted, and which has since been pursued with so much untiring minuteness of industry, may not all be a series of independent facts, produced often by the experimenters themselves, or running only coincidentally, when not produced, with the actual phenomena which Nature presents to the observer. It seems to me the most reasonable of propositions that the blood, in the decompositions to which it is subjected in the extreme parts of the organism—in those vital parts which lie, as I may say, on the banks of the stream of blood—should yield, as our earlier masters taught, a true physical quality which the nervous system should have the power of taking to itself, which should pervade the nervous matter, and which should hold the whole nervous system in one connected bond, while, at

the same time, it should allow of the local independency of parts, and should be subject to impression, not from one kind only of impulse or *vis*, but from any to which it may be subjected—calorific, mechanical, electrical, chemical. It is, in short, essential to all theories of vibratory motions to presuppose the existence of a veritable ether, in which the more solid particles, atoms, or molecules of matter are distributed, and by which, through vibration, the parts of matter are put into motion. Carry, then, this same view to the animal body: suppose a mechanism such as the nervous system, flexible where needed, protected and fixed where needed, central, linear, peripheral, everywhere built upon blood-vessel and blood current, and everywhere capable, where there is vessel and blood, of taking up from blood a product of the decomposition of blood, the resultant of chemical change, suppose this mechanism persistent in the living body, and what do we endow the body with less than with a subtle spirit, which vibrates to every impulse, which during life is dependent on the organism for its development, and which is indeed a spirit, made within the organism, by and through which the outer universe is received, is communicated, is felt, is known? To my view, there is reason in this theory. To my view, the nerves, without the essential physical quality with which they are charged in life, are as the arteries without blood, the silent, emptied channels of what once was a living thing. To my view, nerves may practically bleed during life, as vessels do—bleed, not blood, but a derivative of blood. To my view, what commonly is called nervous exhaustion may be no metaphor of speech, but a physical reality, as definite as that physical hæmorrhage from blood-vessel to which, in effect, in symptom, it is so near akin.

ORIGINS OF ELECTRICAL HYPOTHESES.

When, in 1746, Mr. Cuneus, of Leyden, holding in one hand a glass jar containing water in communication with the prime conductor of a frictional electric machine, unconsciously charged the jar, and, on removing the wire with the hand that was free, got, for the first time in the history of science, a strong electric shock, he, naturally enough, was not the only man astonished. Straightway Professor Muschenhoeck, of the University of Leyden, taking also a shock, writes to Reaumur that the blow he had received, by the new process, through his arms, shoulders, and chest took away his breath, so that he was two days before he got over his alarm, to the cause of which he solemnly declares he would not again be subjected for the whole kingdom of France. But, after Muschenhoeck, other men, more venturesome, repeated the experiment, until it became, for the moment, the great wonder of the civilised world.

When, in a later day—viz., about 1755—Benjamin Franklin illustrated the singular experiment of “knocking down” six men from the discharge of two large jars by laying the end of his discharging-rod upon the head of the first man, the hand of this man on the head of the second, the hand of the second man on the head of the third, and so on to the last man, who held the chain that was connected with the inside of the jars, and when, on making the circuit complete, the men all dropped together, and yet declared they had felt no stroke, heard no crack, seen no light, but wondered why they had fallen, the world was again in amazement.

When, later still—in 1790—Galvani, working his electric machine near to some frogs that had been prepared for the purpose of furnishing sick Mrs. Galvani

(at the suggestion of her physician) with a dish of frog-broth, saw the muscles of the animals thrown into active motion by the inductive action of the electricity of the conductor upon the muscles, a line of research was instituted the results of which so entranced the world that the term "animal electricity" passed into a household phrase, and the idea that electric force and force of life were the same force became, for a moment, an accepted article of belief.

When, in 1803, Aldini, the learned and enthusiastic follower and nephew of Galvani, restored what seemed to be the phenomena of living motion in a malefactor an hour dead on a cold day, the marvel of mankind still increased.

When, once more, in times nearer to our own, the electro-magnetic machine came into use, and it was learned that if the hands be made to grasp the poles of the machine at the time it is in action, the muscles are drawn against the will into vehement contraction, the marvel intensified, and the electrical or electro-magnetical character of the force which animates the living organism became such a strict article of belief, it threw completely into the shade all the reasonings of the older physiologists; had they never been born, they had hardly been more forgotten, more silent men.

It is no fancy or pleasure of mine to under-estimate the intervening chain of great and startling labour which connects the old world of thought with the new. I admit the singular wonder of this fact that I can, by the contact of the poles of a battery, make the muscles of an animal recently dead move again in simulation of the motion of life. I know that when I grasp the poles of the electro-magnetic machine with my own hands I feel as though my will were overcome by a power like

unto its own, but so much stronger that it is subject to the foreign mastery. This is clear enough, simple enough; but is it all? Were it all, the electrical theory of animal motion were indeed complete. We might fail then, as we fail now, to discover the origin of the force within the body; we might fail to discover the animal electrical mechanism in its details, or to strike the comparison between an animal electrical apparatus and the electrical apparatus the human hand constructs for its own purposes; but we could still say, with convincing truth, that the living impulsive force was electrical, and nothing else; therefore the mode of its production within the body must be assumed and unknown as yet, must be left as something which in the course of time will be discovered, described, and even copied, in its design, direct from nature. Here, however, the electrical theory breaks atwain. The phenomena of muscular motion, of sensation, of pain, are not solely producible by artificial methods, through electricity. I return to the forces known only to the pre-electricians, and to other influences than electricity, of which they knew nothing; and the same phenomena present themselves to our attention through these forces. I mechanically irritate the muscles of an animal living or recently dead, and they forthwith contract; I apply to the muscles warmth, they contract: I proceed to treat the nerves in the same fashion, and again the phenomena of muscular motion are before me. I take an animal recently dead, inject through its arterial vessels water of a temperature of 110° Fahr., and every muscle in the body so responds that for a brief interval the animal appears to relive.

I do more than this: I take an organic chemical stance, say nicotine, and injecting it in very minute

quantity into the body of a living animal, I excite a muscular convulsion ending in death; or I make an inferior animal, or man, inhale a minute portion of such a chemical substance as the nitrite of amyl, and at once the blood-vessels are unloosened, and the heart beats doubly, trebly, its natural rate in respect to time; or, once more, I do nothing that seems physical, but I excite in the body through the senses an emotional impulse, and immediately I get a response in disturbed—it may be fearfully disturbed—muscular action. Lastly, if I abstract a sufficient quantity of blood, the inevitable response is severe convulsion; or if upon the brain of an animal I exert a little undue pressure, the effect is declared in convulsive contraction of muscular fibre, universal in its character.

To say nothing at this moment of the production of a negative muscular condition by means of cold, the above-named facts are quite sufficient to demonstrate that neither electricity nor any other initiative force is specially and alone concerned in the propagation of the motion of muscle. It is clear that many of the influences which excite to motion lie out of the body altogether, and affect by contact; some are so subtle—those, for instance, which we call emotional—that we have more difficulty in understanding them than in understanding the action of electric induction: some appear to be within the body, products of a steady-going source of motion, over which we have no determinate control, and which, during the whole of our lives, fan the involuntary organs into living action.

Thus the longer we think of the phenomena of muscular motion—and, indeed, of all motion in the living animal body—the less we are able to regard with favour, on the evidence before us, the hypothesis of one

force in the organism, and of nerves and nervous centres as producers and conductors of that force; while we are the more inclined to extend our relations of life to the universe as a whole, and to take in every motion as belonging to our living receptive organisation. But, in order rightly to conceive the adaptation of the organism to the universe, the ideal of a nervous fluid, a true physical something pervading the nervous system, as the first neuro-physicists taught, is indispensable. It, and it alone, affords the connecting link between force and matter by which force can move matter. Why cannot force—electrical, if we will—move a muscle that has actually passed into the inertia of death?—Why but that the muscle—or, rather, the nervous matter it contains—has lost some physical thing, without which it is dead to motion? Why will not the dead eye see?—Why but that it has lost some physical thing with which it was wont to be charged, and through which the wave of light could extend vibration? Why, when I freeze a part of the surface of the body, will not the frozen part feel?—Why but that in the act of freezing I have condensed or have expelled from the nervous matter of the part the physical agent by which the part was connected, in arrangement and condition, with the same agent in the other portions of the nervous organism? Why, when I make an animal inhale a narcotic vapour, do I produce general insensibility?—Why but that I distribute through the whole nervous system a foreign substance, which interferes with the natural condition for vibration through the nervous matter?

COTEMPORARY PRACTICE AND LITERATURE.

*"Every physician will and ought to make observations from his own experience ;
but he will be able to make a better judgment and juster observations by
comparing what he reads and what he sees together."*—FREIND.

"DIAGNOSIS OF DISEASES OF THE HEART AND THORACIC AORTA."

DR. ERNEST SANSOM, in his new volume bearing the above title (Griffin & Co., 1892), has given to practical medicine an admirable work. I do not attempt to analyse it, and still less do I attempt to condense or epitomise it, but it is a book for every man in active practice to have at hand for reference, and to which, when he is in doubt in diagnosis, he may appeal with the assurance that he has for his assistance not simply an industrious, observant, and experienced, but, best of all, a conscientious, author. Sansom knows difficulties, and meets them with a patient understanding which is almost unique in character. There are many points of detail, many explanations of his, on which it would be fair to differ with him, and of that he is self-conscious ; but the general tone of his book is knowledge, and its outcome wisdom.

The volume is copiously illustrated, and although the sphygmophone has no place in its pages, the sphygmograph plays an important part, the tracings, somewhat unequal in value, adding largely to the

practical value of the treatise. For a good many years to come this volume will no doubt remain a standard work, and it cannot fail to do credit to that English school, which, founded by Hope, has ever held first place in cardiac diagnostic art.

"LEPROSY AND VACCINATION."

MR. W. TEBB, the writer of so many trenchant articles against vaccination, has furnished us with a compact volume (Swan Sonnenschein & Co., 1893). His contention is that leprosy has greatly increased during the last half-century; that it is not a contagious disease in the ordinary sense of the term, but is communicable by means of a cut, sore, or abraded surface, a view he has confirmed by his own personal investigations. He asserts that malaria, fish diet, syphilitic cachexia, heredity, and insanitation, are unequal to explain the rapid growth of the disease in certain of our Crown colonies and dependencies as well as in other countries; urges that there is one point on which there is hardly any dissent, namely, the inoculability of leprosy; and insists that this view is older than the bacteriological speculation of origin. On this he insists that the most frequent opportunities of inoculating the virus of leprosy are afforded by the practice of vaccine inoculation, which is the only inoculation habitual and imposed by law, and that the evidence he adduces is calculated to show that vaccination is a true cause of the diffusion of leprosy. In support of his argument Mr. Tebb points out that the authorities are not blind to the dangers of vaccination in relation to the spread of the disease, but that they think the danger lies in

vaccinating with an impure lymph. Thereupon he turns round with the formidable question, What is lymph when it is pure, and by what standard, chemical, physical, vital, can any one judge of the purity of such an article? Here, undoubtedly, Mr. Tebb is at home, and candid as he is in controversy as well as courteous, he laughs his fill. The book is well worth careful study.

SULPHUR TOXÆMIA.

ULPHUR toxæmia, as a condition of disease from sulphur compounds, I have elsewhere described, is now claiming an attention which I wonder has not been claimed before. In the *Medical Magazine* for January last, Dr. A. C. Farquharson writes an able article entitled "On Organic Sulphur Compounds in Nervous and Mental Diseases," and discusses the subject with great earnestness. He thinks that the common tie which ought to connect the symptoms characteristic of poisoning by sulphur is broken when comparisons are instituted between the symptoms produced by one organic sulphur compound and another—between the symptoms, for instance, excited by mercaptan or sulphur alcohol and those produced by sulphuretted hydrogen—and that "while differences so great may, for speculative purposes, be grouped together under the comprehensive phrase of mental derangements, they are still as wide apart as to suggest etiological factors beyond sulphur compounds, and to justify the contention that the one cannot be taken as corroborative of the other." I admit the fairness of this contention, and am not surprised to see that it meets, tentatively, the wise and thoughtful editors of the *Journal of Mental Science*, who, in their

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he worked with a single microscopic lens no better than the half crown lenses we meet with on the toy stalls of our day. Certainly he had nothing so good as the microscope which his less learned contemporary, Leuwenhoek, ultimately produced.

In 1668 Malpighi was made member of the Royal Society of London, and the next year 1669, then appeared in the publications of the Society his Treatise on the Natural History of the silkworm. This was at the time Malpighi held the professorship of medicine at Messina.

Eight letters and treatises were submitted by Malpighi to the Royal Society, and in 1686 his complete works in two volumes "Opera Omnia" were published by the Society. The 1st vol. contains the 1st part of the Anatomy of plants and various letters to Oldenb and Spon. The 2d. vol. contains the 2d. part of the Anat. of Plants, dissertations & letters treating on the silkworm; the formation of the chick in the egg; exercises & letters concerning the brain, tongue, external organs of the skin

foundation of the work lies in the practice at the General Hospital for Sick Children at Manchester, and the peculiarity of it, the novelty of it, is that it is written by two observers, one a physician, the other a surgeon, so that medical and surgical experience play equally their important parts. The first chapter, on the physiology of infancy and childhood, is written with remarkable care, and shows well the fact that in studies of physic, as in many others, two heads may be wiser than one; for it is a chapter full all through and strong all through. The authors must be congratulated on the clearness of their style, their freedom from the use of ambiguous words and phrases, their avoidance of long-winded expositions, and their care in delineation of illustrative drawings and plates.

THE ANTISEPTIC TREATMENT OF TYPHOID FEVER.

DR. A. M. ANDERSON has published a concise and very straightforward essay on the antiseptic treatment of typhoid or enteric fever. He bases his treatment entirely on the antiseptic hypothesis, and the drug he depends upon is salol, which he administers most freely. He furnishes us with a list of ten cases which went through the ordinary course of typhoid. Of these ten three died and seven recovered, so that the mortality following the treatment was at the rate of 30 per cent. The author is to be commended warmly for the candour and earnestness he manifests on every page. It is no part, he says, of his thesis to maintain the omnipotence of antiseptics in typhoid to save every life attacked, nor yet to contend that his method of antiseptics is perfect in every detail: but he is firmly

convinced, and stoutly maintains, that the antiseptic treatment disclosed, and which, as far as he knows, is original, when it is properly applied on or before the tenth day of the disease, is infinitely the most powerful and rapidly effective agent ever yet tried in the treatment of typhoid. He also shows satisfactorily, how high temperature can be brought down below the normal by the use of the remedy. In brief, his hypothesis is ably sustained by everything except the results of the treatment. The mortality was very high, and I would not, for my own part, publish the praise of any method that yielded me the same results. Compared with the tepid bath treatment, it is in bad position altogether, but Dr. Anderson has performed a valuable service in giving us his experience and in adding to our literature what is distinctly a new chapter in the history of therapeutics bearing on the treatment of one of the worst scourges of disease we have to meet.

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*THE LANCET AS AN INSTRUMENT OF
PRECISION IN MEDICAL PRACTICE.*

IF it be true that travelling about in the world brings strange bedfellows, it is equally true that a long journey of medical life brings strange experiences. The fact is strong in my case as bearing on the use of the lancet for the abstraction of blood in the treatment of disease. During my earliest days of practice, I was assisting for a few months a firm of medical practitioners in the Midlands. I found very pleasant society there, and made the acquaintance of a family to one of the members of which an accident occurred. This acquaintance, a gentleman, returning from market to a country village, was thrown out of a gig, and sustained a severe injury to the head by which he was rendered unconscious. A surgeon was called in to see him, and was expected at once, according to the current practice of the day, to draw blood from the arm of the injured man. He declined, nay refused, and, although the stunned man recovered, was subjected to great annoyance for doing what he conscientiously believed to be right. It was urged by the family,—backed up, I fear, by the professional opinion of an antiquated member of the family,—that if blood had

been boldly and freely drawn there would have been quick and perfect recovery. Hence arose, in the end, a serious disagreement, and threats of an action against the unfortunate surgeon for malapraxis. At this juncture I was spoken with by those who thought themselves aggrieved, but having much sympathy with the surgeon, I did my best to persuade the friends to let the matter drop. I explained that the opinion of the profession about blood-letting was undergoing a change; that the new views were probably correct; that the untoward symptoms would in all likelihood have appeared if blood had been drawn; and that they would in the course of time pass away. To be brief, the matter went no further, and I received a note of warm thanks from my medical brother for having saved him a very disagreeable ordeal, even if he had come off victoriously, a result, according to the genius of juries in that day, more than doubtful.

Quite a period of four decades passed away, when one morning there appeared in my consulting room an indignant father, who was about to bring an action of malapraxis against another medical man for reasons just the opposite. He told me he had a daughter who had suffered from scarlet fever; that after the acute symptoms of fever had subsided she became dropsical and drowsy; and that the doctor in attendance, declaring the act to be positively necessary for the saving of her life, had bled her. She had slowly recovered from the dropsy and sleepiness, but "had been left bloodless," and was considered in a hopeless state by more than one authority. In plain words, he concluded, she had been practically stricken down by the bleeding; and, as he had been assured that the error of the process was unpardonable, he had determined to proceed against

the supposed medical offender, and wished to know if I would give evidence in a court of law in support of his argument. Declining at once, I explained to my visitor the state of the case: that bleeding under the circumstances he named was scientific treatment; that in all probability it saved his child from immediate death; that, had I been consulted, I should have done the same thing; and, that in evidence I could supply examples in which I had done the same thing with the most perfect conviction that under the circumstances it was the best to do. The result was that the thought of legal procedure was withdrawn. In due time the young patient made a fair return to health.

These sharp contrasts of professional and public opinion on the value of a great remedy are useful as indicating how charitable we all ought to be on the question of innovation in the art of medicine. What thousands feel now against the practice of abandoning alcohol as a remedy, as many thousands felt half a century ago against the abandonment of blood-letting as a remedy. These shocks of opinion are most strange, are open to serious comment, and often expose men like myself, who wish "to prove all things and hold fast that which is good," to grievous misunderstanding, because of our "provoking obstinacy" against the acceptance of rapid changes of doctrine and our lingering support of what are too easily called antiquated prejudices. I name the facts as preliminary to the statement that, according to my latest view, the lancet still admits of being used as an instrument of precision in medical practice, and that, used in such manner, it sometimes is one of the most powerful aids in the saving of life at moments of critical danger. The view is perfectly consistent with the admission of

the truth of Lord Byron's criticism, "More men die by the lancet than by the lance," for when the lancet was all-powerful, it was used so indiscriminately, it could not fail to do some evil in the long run. The great practical question is, Did it do all that was wrong, and nothing that was right?

I have said that the lancet did harm, and an essay by the distinguished Chomel proves this. He reported in relation to an epidemic of scarlet fever that he bled his patients *coup sur coup*; they became convulsed; he bled again, with further convulsions and death. He did not seem to have been cognisant of the simple fact that hæmorrhage leads to convulsions, and that he was producing hæmorrhage. His mortality, attributed to the virulence of the fever, but evidently due to the treatment, was alarming; still he appears to have been content, and, under the delusion of his day, to have felt that everything was done to save life which human skill could do.

There was witnessed in this history the extreme faults of a too exclusive practice; and from such data, in these days, many are wont to believe that all errors of the same kind were once common, and that every one who was bled was injured. The last is a false conception. I saw many bled, and was not too late to take part in the "spring-and-fall" bleeding business; yet I do not remember to have seen any one injured by the abstraction of blood. They who fell into the habit of being bled spring and fall were healthy people, and after the abstraction of even a pint of blood in some cases, went on with their daily work as though nothing had happened. The rule was to bleed until the patient just felt the effect, and then to stop. To feel the effect was to become a trifle faint, without actual

deliquium. The recovery to the normal equilibrium was very soon accomplished.*

Bad effects from the periodical blood-letting my seniors all ignored; some of them indeed believed it was a good practice, and that the symptoms of certain diseases, of epilepsy especially, "were staved off by it."

I do not remember ever to have seen any very serious symptoms following upon venesection performed for the treatment of disease. It was astonishing how rapidly even thin and anæmic patients recovered from the

* The last experience I had of the spring-and-fall business was amusing, as many serious subjects may be. I was living at Mortlake, taking part in a practice in which in days gone by the spring-and-fall blood-letting had formed an important item in the large income that had been received by the firm. One of the last spring-and-fall men came to me, or, as he would have said, "to the surgery, to be blooded as usual." I recall him well as a bold, strong, honest, rough-looking farmer from Turnham Green, where farm-houses then stood alone in their glory. He got off a strong horse, rough-and-ready as himself, the saddle rough too, with leather torn not a little, and hair padding sticking out. He said with a loud voice to the animal he dismounted from, "Wo, Putty! I'm going to be blooded"; pulled the old reins over the head of the animal; tied them to the garden gate; walked, knowing the place well, straight into the little room where morning work was carried on, and sent in his name to me, asking the servant, by the way, "whether the new doctor wor as kliver as the old 'un." When I came to him he had got his coat off, and had put round his left arm in the right place the fillet of tape he had brought with him. He could not for the moment speak, because with his right hand and his teeth he was tying tight the knot of the fillet. This done, he closed and opened his herculean fist a few times, by which he raised the vein in the bend of his elbow into a full cord, which he touched, to try its tension, in quite a workmanlike and admiring manner. Then, all being ready, he condescended to notice me.

"Good-mornin', sir. I've come as usual to be cleared out a bit. I think that 'un as good as I ever seed" (pointing to the vein). "He'll bleed like a pig, he will."

I, somewhat surprised and amused, took up his right hand, counted his pulse, and replied,—

deliquium of blood-letting. I recall that in riding through the village of Radwinter, in Essex, I was called in to see a tailor, a man of feeblest type, who was suffering extreme pain from acute pleurodynia. As a matter of course, I tied up his arm, and bled him freely, with instant relief to the pain. This was what Galen would have advised and carried out, and the result justified the means. The blood was neither buffed nor cupped, and that one bleeding was a practical cure. There was a little faintness towards the close of the bleeding, but no other bad effect.

"My good sir, you don't want bleeding; your pulse is excellent, and you are quite well."

"I don't want bleedin' i' the spring, do you say? Why, I don't b'lieve I ever missed it sin' I wor born."

"Perhaps so, but you do not require it; it can do you no good."

"You won't do it?"

"No!"

"Perhaps yer not up to it; yer look young."

"Oh yes, I can do it well enough!"

"Do yer think I don't mean to pay yer, then?"

"I never thought of such a thing; I am sure you would give me my fee if I did the business."

"That's square anyway, but why won't you do it?"

"Because I don't believe in it."

"Not believe in spring-and-fall bleeding, as was done years afore you were born?"

"No, certainly not!"

"Then I'll go to some 'un who has had more experience. Good-day to yer!"

And so he left; and scrambling into his old saddle, he plunged away to a rival Æsculapian. Half an hour later, as I stood at the window looking into the street, he returned exultant. He pulled up to a slow shamble as he went by, pointed to his left arm to intimate to me that the thing he wanted was accomplished, grinned a triumphant grin, and making me a sign which I now know was not strictly masonic, pursued his homeward way, looking upon me, I daresay, as one of the stupidest doctors he had ever met.

The above is one of many illustrations which I have retained in my clinical notebooks. Some I have published in years gone by, a few of which are well worthy of reproduction now as affording to modern medicine a brief view of the practice of an earlier date.

EFFECT OF BLOOD-LETTING FOR CURE OF HEAT SHOCK.

An early impression has probably held me more closely than most medical men of my time to the practice of venesection. The impression to me is as vivid now as it was when it was first made, over fifty years ago. Into the garden of a surgeon,—an uncle who gave me some of my earliest lessons in the art of physic,—there was brought one extremely hot day a man who had been struck down by the sun's heat in the reaping-field. The man was carried by two or three of his comrades, as a dead man might be. He was pale, speechless, unconscious, and rigid. I remember that the stiffness of the man struck me as most remarkable, and filled me with alarm, for when he was placed in an arm-chair there was a difficulty of bending him into it. He showed, in fact, what we in this day recognise, through experiment, as spasm from heat; and we are aware now that this means a condition of extreme danger. Without a moment's hesitation as to the practice he should pursue, the surgeon tied up both arms of the man for venesection, and bled from both at the same time. For a brief period there was a feeble flow of blood; then the current came in full stream, filling quickly two pint measures with blood as bright red as if an artery instead of a vein had yielded it. Under this abstraction, the muscles of the man relaxed; he breathed deeply, with a sighing expression, looked round, and then broke the silence by saying to

me, holding one of the basins, "You're a good boy!" and from that moment recovered his consciousness. The flow of blood arrested and a bandage applied, this man got up from his chair, and walked to his home. He positively never came back to report himself, but I met him driving a cart a few days afterwards, sitting on the shaft and smoking his pipe, as well as if he had neither been brought to the gate of death by accident nor pulled from it by bold and scientific skill.

EFFECT OF BLOOD-LETTING FOR CURE OF MECHANICAL SHOCK.

While serving a term of articleship with Mr. Henry Hudson (formerly in practice at Somerby, Leicestershire), I went with him to see some pony races held at Burrow Hill, a place famous as the remains of a Roman camp and well known to the antiquarians. The racecourse was flanked all round on its outside by a rising ground, once an embankment or rampart, and on this several hundred spectators had gathered together. During one of the races, while the ponies were running their final heat, a woman, about forty-five years of age, rushed down the embankment to pull back a child that she considered was in danger. The impetus she got in the quick descent prevented her from stopping at the foot of the embankment, and projected her into the course just as the riders were passing the spot. She was knocked down, and was so stunned that when she was picked up she was, at first, considered to be lifeless. In a little time it was detected that she was breathing, and so she was conveyed from the hill to an inn in the village of Burrow, where Mr. Hudson, taking me in company, attended her. She was put to bed immovable and unconscious, with her eyes fixed, and her breathing

deep, noisy, and slow. I recall perfectly the observation that the pulse was all but imperceptible at the wrist, and that the veins of the arm filled very slowly, when, according to the orthodox practice, the fillet was applied, and a vein was opened. Blood, which but trickled at first, began to flow as the hand of the patient was pressed and moved; slowly the stream of blood increased, and at length became full. When the previously almost stagnant blood began to move, and the heart found relief from the pressure to which it had been subjected, the phenomena of active life recommenced. The deep breathing became shorter and quicker, the pulse at the wrist rose, the muscles began to move under a half-conscious volition, and before we retired the patient was so far recovered as to be able to swallow liquid food. We left her feeling she was safe, and we were justified in our hopes; for, although she had lost a pint and a half of blood at least, she suffered nothing from the loss of it, and in a day or two returned to her home and to her ordinary duties, perfectly restored.

EFFECT OF BLOOD-LETTING FOR THE CURE OF LIGHTNING SHOCK.

Our forefathers were satisfied as to the good effects of blood-letting in cases of lightning shock. Dr. Macaulay, an able naval surgeon of last century education, has left on record the history of a man who, struck down on deck by lightning and being entirely insensible, was brought to consciousness and recovery by the rapid abstraction of over forty ounces of blood. I have not myself had the opportunity of treating a case of lightning shock in the human subject, but an experience of another kind bears directly upon the value of the remedy in such cases. In experimenting with the

great induction coil at the old Polytechnic, I tried to kill large animals—sheep—painlessly by an electrical discharge derived from a Leyden battery set “in cascade,” and presenting ninety-six feet of surface. This shock is identical with the fatal intense shock of lightning, and by passing it once through the body of a sheep it rendered the animal instantaneously unconscious, to all appearance dead, and, as I found by one line of experiment, actually dead, if nothing more were done to it. But in another line of experiment the animals so soon as they were stricken were removed by the butcher, and were subjected to division of the vessels of the neck in the usual manner of killing in the slaughter-house. At first blood flowed very slowly from the operation, but in a short time the current became freer, and, as it became free, the phenomena of active life, previously suspended in the animals, returned. There was, for a moment or two, return of consciousness, of motion, of struggle, and those proofs of life that an animal passes through, previous to convulsion, when it is submitted to slaughter without shock.

If we connect the experience of those of our predecessors who have successfully employed blood-letting for the cure of lightning stroke with the experimental facts I have here adduced, the inference is, I think, as fair as inference can be, that blood-letting is the remedy for the effects of the shock of lightning, experiment, equally with experience, becoming of clinical value.

EFFECT OF BLOOD-LETTING FOR THE CURE OF SUDDEN PNEUMONIC CONGESTION.

In the year 1847 I attended, for the late Mr. T. Brown, of Saffron Walden, Mr. C., a gentleman farmer, who then lived at the village of Elmdon, and who has but

recently died. This gentleman, while suffering from a severe catarrh, or, as it was called, an "influenza," was suddenly exposed to an extreme degree of cold, which prostrated him at once. As the freedom of the circulation became restored upon reaction, the influx of blood into the paralysed vessels of the minute circulation led to that stage of congestion which in my lecture on the influence of extreme cold on the peripheral surface (*Medical Times and Gazette* for May 11th, 1867) I have placed as the third stage in the process of recovery from the local action of cold. The result was that both lungs became suddenly and universally congested, rendering his death imminent. I went to him in the night, and had soon the pleasure of being joined by Dr. Paget, of Cambridge, who had been specially sent for. The patient was breathing with great difficulty; his face was bronzed, his mind a little beclouded, his pulse hard. There was general dulness of percussion over the thorax, and commencing fine crepitation in the posterior part of both lungs. Now and then, as I sat watching him, waiting for Dr. Paget's arrival, I noticed that, with a frequent accession of laboured breathing, there was a convulsive action, short in duration, but well defined. Dr. Paget, appreciating the extreme danger of the patient, suggested, as the most instant remedy, a free venesection. At once, and while he remained, I carried out the practice liberally, and, I hesitate not to say, with immediate result for the benefit of the sufferer. Practically this sick man was placed immediately, by the remedial art, out of the extreme danger into which he was cast. "Take a little more; I am so relieved by it," was his observation as I sought Dr. Paget's indication, from the pulse, when I was to stop the flow of blood; and

with that wish we thought it reasonable to comply. The relief to the oppressed breathing was direct; the face lost its bronzed hue as the better-aerated blood made its way round the body; the extreme tension of the pulse ceased; the secretions were set at liberty; and the progress of the patient to recovery was as satisfactory as it was rapid. I had the pleasure of seeing Mr. C. many years afterwards and of hearing from him an often-repeated statement of his gratitude for the prompt skill that saved his life.

EFFECT OF BLOOD-LETTING IN PROGRESSIVE PNEUMONIA.

In the year 1851 a case came under my observation at Barnes. It was that of a strong man, about twenty-six years of age, who suffered from pulmonary congestion, induced by exposure to cold and wet, with exhaustion from want of food. He had been to London on a visit of "pleasure," got wet through during his journey to town, stood about in his wet clothes all the day, and finished up by going to the play and returning home on foot. This occurred on October 15th, and on the 16th he had what he called a cold. He continued to grow worse, and on the fourth day I was called to find him suffering from congestion of the left lung, with fine pneumonic crepitation at the posterior part. He was treated with salines, and was sustained, as far as possible, by food, but he did not progress, and on the sixth day a dozen leeches were applied to the chest, the dyspnoea having become very severe. A few hours later, at my evening visit, I discovered that the congestion had extended to the right lung. The face now was bronzed, the breathing very difficult, the action of the heart laborious, and the pulse tense. The patient was, in fact, in the same condition as Mr. C., the

history of whose case has been given immediately above, but with this material disadvantage, that he had struggled through a progressive course of the disease, had fasted longer, and was feebler. I determined, nevertheless, to take blood cautiously from the arm, and when I had drawn rather over ten ounces I was obliged to desist, owing to the occurrence of faintness. This condition of faintness was of short duration, while the relief gained by the abstraction of the blood was most perfectly demonstrated in the instant freedom that was afforded to the hard and laborious breathing, to the oppressed heart, and to the loaded vessels. From this time improvement was steady and sure, and ten days from the commencement of the attack the man was convalescent.

EFFECT OF BLOOD-LETTING FOR THE CURE OF COMA AND CONVULSION AFTER LABOUR.

In the close of the year 1847 I was called to the village of Radwinter, in Essex, to see a young woman who had recently been delivered of a first child. A midwife had attended her, and had rendered every necessary assistance. The labour had been marked by only one striking peculiarity—viz., the enormous quantity of liquor amnii that escaped; the placenta had been thrown off naturally, and the uterus had contracted well. For some days preceding her labour the woman had been unusually dull and oppressed, and within an hour after the delivery comatose symptoms with convulsions set in. At the time of my arrival there was complete unconsciousness, with stertorous breathing, strabismus, and active convulsions recurring every fifteen or twenty minutes. The patient was quite unable to swallow, and I found that free counter-

irritation, in the way of a turpentine stupe, had been applied without result. To bleed in this case, and to bleed freely, was the practice I had been taught; and therefore I carried out the practice without delay. I drew off one pint of blood, with immediate effect in stopping the convulsions; but as the coma continued, I ventured, at the end of an hour's time, to repeat the venesection, and drew off nearly a pint more blood. After this I waited until, from a restless state resembling the awakening from sleep, the woman became partly conscious. I left directions that she should be fed moderately, and should have no stimulant. I also directed that the child should be put to her breast so soon as she was able to take it by her side. On the following day this patient was perfectly conscious at the time of my visit. She had suffered no return of convulsion, and she was not feebler than women generally are after confinement. She made a rapid and perfect convalescence, without resort to any other medical treatment than the venesection.

EFFECT OF BLOOD-LETTING IN A CASE SIMILAR TO
THE ABOVE.

On May 29th, 1873, the late Mr. Burton, of Richmond Terrace, West Brompton, summoned me to see a lady whose symptoms were nearly the same as in the case immediately described above. In this lady the most marked peculiarity of the labour, so Mr. Burton told me, was the excessive discharge of amniotic fluid. After delivery she became comatose, insensible, and vehemently convulsed. Her face was congested, her veins tense, her pupils fixed, and her temperature high—103° Fahr. Before my arrival the patient had been bled freely from the arm, and the more alarming symptoms had

subsided; but the jugular veins were still tense, the temperature was 102° Fahr., and the unconsciousness continued, with an occasional convulsion. Confirming fully the treatment that had been pursued, I recommended the abstraction of still more blood by leeches, together with continued application of a collar of ice around the neck. I learned afterwards from Mr. Burton that the amendment in the symptoms continued, and that recovery took place without any interruption. Mr. Burton was so good as to take me to see this patient some months afterwards. I found her quite well, and I could gather nothing from her that could lead to the remotest suspicion of her having suffered the slightest harm from the remedy which, I am morally sure, saved her life.

EFFECT OF BLOOD-LETTING IN URÆMIC COMA.

In the close of the year 1850 I attended a boy, seven years of age, for scarlet fever. He recovered from the fever, but in January, 1851, he took cold on being exposed to wet, and on January 21st I was called, to find him in a state of profound uræmic coma. Some urine he had passed was rich in albumen; his body was cold, his pulse small, his consciousness altogether lost. Convulsions of a severe character had set in, involving the muscles of the neck, the face, and the limbs. The pupils were widely dilated and fixed; the breathing was catching and irregular. I tied up an arm and opened a vein. The blood at first flowed feebly, but by-and-by came in a full stream. Under this depletion, the pulse rose, and the convulsions ceased. I took sixteen ounces of blood without producing a sign of faintness in this little patient, and I left him composed and conscious. A purge was

afterwards given, and occasionally a warm bath. On February 1st he seemed to have recovered entirely, and the urine was free of albumen.

It is worthy of note that this boy, after passing some weeks in good health, playing, eating, and sleeping naturally, while finishing a too hearty meal, was again seized with convulsions, and died. In this fatal attack he was not submitted to venesection, his death occurring before the operation could be performed.

LEECHING IN A CASE OF URÆMIC COMA.

In the beginning of the year 1873 I was called to a young lady, eleven years of age, who, after an attack of scarlet fever, was seized with uræmic coma. I found her partly unconscious, with convulsions, with slight strabismus, with the pupils widely dilated, and with the temperature of the body normal. The urine she passed was richly albuminous. I suggested the immediate abstraction of blood from a vein, but was so strongly opposed, I did not urge the practice. As the symptoms in a few hours became much more imminent, consent was given that I might take blood by leeches. I seized the opportunity, and I carried out the bleeding as effectively as if blood had been taken from the arm, removing by relays of leeches applied over the loins fully eight ounces of blood. The relief afforded was immediate; and, the temporary congestion relieved, the patient continued to improve until she made a perfect and rapid recovery.

VENESECTION IN A CASE OF SO-CALLED APOPLEXY.

In 1847 I was attending for Mr. Brown, of Saffron Walden, an aged man who lived near to Littlebury, in Essex. This patient had for some time complained

of symptoms of suppression of urine, with drowsiness, which symptoms I knew practically were sometimes removed by a purge. Late one night I was summoned to see him because he had been taken in "a fit." He was profoundly comatose and frequently convulsed. I knew then very little about uræmic coma, and I was beginning to share in the general panic which was seizing the profession respecting the manifold dangers of blood-letting. So I refused—though the friends of the sufferer, good old-fashioned people, remonstrated with me—to bleed. In lieu of bleeding, I ordered a blister to the nape of the neck, applied cold to the head, and tried ineffectually (for the man could not swallow) to administer a purgative. I expected nothing less than a fatal termination, and left with that honest impression. The next morning I received a message not to visit this patient again, "as another doctor had been called in." Having to pass the house, however, a day or two afterwards, I ventured, notwithstanding the prohibition, to make an inquiry as to the state of the sick man. He was sitting up in bed perfectly conscious; and a day or two later I saw him leaning on his staff at the door of his house, as well in health as he had been for a period long past. I have learned since that the old gentleman lived several years after I had left the neighbourhood, and that he had many a laugh at my expense, which in truth I deserved quite as richly as he enjoyed.

The history of his recovery is full of interest. His friends believed him to be suffering from "a stroke of apoplexy," and having formed that reasonable diagnosis, they came to the practical conclusion that he ought, therefore, to be "blooded." As I left him, giving no hope and refusing the desired remedy, they called in a

neighbouring medical practitioner who soon afterwards was seen riding past their house, and asked him to give his aid. The practitioner thus summoned—the late Mr. Alfred Jones, of Saffron Walden—had none of my fears. He trusted the lancet, tied up the arm of the dying man, and drew blood freely. With the flow of blood the cloud of unconsciousness began to clear away, the convulsions subsided, and perfect recovery crowned the treatment.

I was on the wrong side of the fence that time, and, at the moment, felt sufficiently humiliated; but I profited by the lesson, and have never regretted the humiliation.

EFFECT OF BLOOD-LETTING FOR THE RELIEF OF SPASMODIC PAIN.

Our forefathers were wont to resort to the abstraction of blood in cases where there was acute spasmodic pain, as during the passage of a gall-stone or of a stone in the ureter. We have other resources now at command for controlling this class of painful affection, for the anæsthetics are at our service with which to soothe, and, if they be carried far enough, to relax muscular spasm. Whether these remedies are better than the old one and safer, it were difficult to say. The following case is typical of the effect of venesection for the relief of pain from gall-stone:—

A man, forty years of age, short, and of full habit, who lived freely and drank his full share of old ale, was attacked with symptoms of congestion of the liver, with slight jaundice. He was treated for this by the late Mr. Edward Dudley Hudson, of Narborough, one of the most admirable practitioners of medicine I have ever known in my time. The symptoms abated, and

the man recommenced to follow his usual mode of life. In the middle of a day, a few weeks later, this man was suddenly seized with the acutely painful symptoms caused by a gall-stone passing the gall-duct. A messenger was despatched for medical assistance, and I was soon in attendance. I found the man with his body bent forward over a small old-fashioned table, the pressure upon which seemed, he thought, to give him some temporary relief. The pain was all referred to the region of the gall-bladder; its cause was sufficiently clear, and its intensity I have never seen more severely developed. Anæsthetics at that time were just known, but I had never heard of their application for this particular malady; so I did what nine medical men out of ten would then have done: I had the man placed in an upright position, and bled him until he became faint. To produce this effect in the position named, not more than sixteen ounces of blood had to be drawn, and so soon as the effect was established the pain was gone. Under the action of a purgative, administered the next day, the patient passed by the bowels a biliary calculus. He made a good recovery.

In these days we should treat a patient suffering as this patient suffered with narcotics, carrying the narcotism up to the degree of removing pain and causing muscular relaxation. I have been compelled myself to treat in this manner. It is not many months since I sat up all night with a patient and colleague who was writhing in the agony of passing a gall-stone, and to whom the hot bath and a narcotic, repeated over and over again, were of little service until the relaxation of exhaustion, from pain, gave that relief which the loss of a few ounces of blood would have ensured in a few minutes. But my hands were tied from the use of the

grand old remedy, and I could only look on to regret that I was forbidden to follow out a practice which, at one time, it would have been considered a cowardly fault to have omitted.

EFFECT OF BLOOD-LETTING FOR RELIEF OF OPPRESSION
OF BLOOD IN THE HEART; CARDIAC CONGESTION.

There are cases of disease in which the right side of the heart of the patient, enlarged and enfeebled, becomes easily oppressed with the weight of blood, and fails to lift its charge freely over the pulmonic circuit. The symptoms under such circumstances are extremely severe. They are compounded of dyspnoea and syncope, with a sense of impending dissolution and oppression at the chest which is indescribable. The veins are tense of blood, the face livid. It was, I doubt not, in these cases that the ancients observed the strange phenomenon of the face of the sufferer becoming of natural colour under the loss of blood.

A patient suffering from these symptoms, and who had reached the age of eighty-four years, was under my observation for several months. She suffered two or three times a year from attacks of cardiac congestion, which, according to her view, were asthmatical, but which were, in fact, due to that weakness and dilatation of the right side of the heart described above. When she was first afflicted she had been treated by the abstraction of blood, and with such success, she would never allow any other treatment, when the paroxysm was more than usually severe, until that remedy had been resorted to. She had herself no fear whatever about the loss of the blood, and I knew her subjected to it three times in one twelvemonth. The abstraction of from six to eight ounces invariably

brought her relief from the oppression; and "Now I breathe again" was her usual remark as her circulation was restored to its natural course, and her anxious, dark, congested face became clear and natural. This lady died at last from simple senile decay.

EFFECT OF BLOOD-LETTING IN PULMONARY HÆMORRHAGE
WITH HÆMOPTYSIS.

On June 11th, 1852, Mrs. C., a woman approaching the climacteric, and who had not menstruated for two months, was seized, while walking out of doors, with difficulty of breathing and with vomiting of frothy blood. She was taken to her home at Barnes, and I, being then at Mortlake (near by), was soon in attendance. At the time of my arrival the patient had vomited eight ounces of red frothy blood; there was general and extreme dulness over the whole of the left lung, but the right lung was still clear, and she breathed by it with sufficient freedom to continue long to live. Her face was dark and congested, she was nearly pulseless, and her extremities were cold. I had her feet placed in hot water with mustard added to it, and sent for my friend Dr. Willis, of Barnes, with whom I was at that time connected in practice. On his arrival, our opinion being the same—that Mrs. C. was suffering from sudden pulmonary congestion or apoplexy of the left lung, and that the hæmorrhage was a sign of the congestion—we determined at once to abstract blood from the arm, and drew off three ordinary teacupsful. On this a tendency to faintness of the patient occurred, but the symptom was trivial, while the relief to the congestion was immediate. The breathing became freer, the hæmorrhage from the lung ceased, and there was reaction, followed soon by a free expectoration of

bronchial mucus. In a word, the imminent symptoms, promptly met, were promptly relieved; and recovery, although retarded by an external perineal abscess, which held her back, was completed during the ensuing month of July. In no case I have recorded was the immediate good effect of blood-letting more marked than in this case. It was as distinctly recognised by the patient herself as by us who were ministering to her relief.

EFFECT OF BLOOD-LETTING IN APOPLECTIC COMA.

In 1870 I was called by my friend and neighbour Mr. Roberts, of St. John's Wood, to see a gentleman of middle age, who was lying in a state of apoplectic coma, with recurrent convulsion. The patient was unconscious, his breathing was stertorous, and his temperature, which had been high, was beginning to fall. His bronchial tubes and trachea were loaded with fluid, his eyes were fixed, and his pupils were contracted. To our minds, and to the minds of all who were present, this patient had not many minutes to live; but as we had permission, in the way of a last resource, to bleed, we carried out the practice of bleeding *pleno rivo*. A vein in the arm was freely opened, and the blood coursed out of the opening in full and high current. At first the blood was very dark in colour, but when twenty ounces had been drawn it came over of a brighter colour. The convulsions ceased, and the patient, for the first time in some hours, gave evidence of consciousness by coughing and spitting up freely the fluid that filled his trachea and bronchial tubes. When twenty-four ounces of blood had been taken, the pulse, which had previously been hard and resistant, became soft; the veins became less tense, and

the pupils relaxed. The flow of blood was now stayed. The patient slept without stertor, and the next morning, when I visited him with Mr. Roberts, I saw him move from one bed across to another and take part in changing his dress. He recovered from this attack with great rapidity, and felt the loss of blood so little that in a day or two he was able to leave the house.

These are records of old practice. In a further communication I shall add some of a later day, and shall offer one or two commentaries of a practical kind, indicating what particular conditions and symptoms call for abstraction of blood by the lancet. I shall also add a few pages on the subject of the local abstraction of blood in the treatment of disease.

*THE HEALTHY CULTURE OF THE LITERARY LIFE.**

THROUGH the greater part of my career it has been my duty to make special observation on the health and life of many members of both sexes who have been engaged in literary occupations. The observations have been founded to some extent on what may be called "fellow-craftship," for literature has ever been to me a field of labour; but they have also and chiefly been founded on professional communications of a medical kind, by reason of important official positions which I have long held with the craft-ship. Under these circumstances it would be easy for me to construct a long essay, nay even a volume, on the subject of health under literary labour, if time for the effort were at my disposal. Time sufficient is not at disposal, and I propose, therefore, at this present, to deliver what may be called an abstract of the larger project, and that, too, very briefly and tersely.

LITERARY WORK IN ITSELF HEALTHY WORK.

According to my experience, literary work is work of the healthiest kind when it is carried out with proper care and provision. It affords also a class of occupation which any man or woman blessed with common industry

* An address delivered to students of the Birkbeck Institution at the opening of the winter session 1877, and not before published.

and common sense can follow, the field for it being as wide as the wants of man himself, a field which offers nothing more and nothing less than all life, or any chosen part of life, for its development.

To begin at the beginning, let me say that I can see no difficulty in any man or woman of ordinary capacity becoming a man or woman of letters and following literature as a profession. All will not become geniuses; very few will, for to the goal of genius there is no royal road; but towards fair position in literature every honest and industrious man or woman may strive with just hopes of success as the reward. In every case any genius that may be latent will have its chance of development, and probably many geniuses will be discovered who would not have been heard of if literature had not tempted them into her fields of labour.

Let it not be understood in saying so much that I am suggesting an easy life in a literary life. Far from that; the life faithfully pursued is a laborious life. If it be carried out in the anonymous phase of it, it is a weary toil for which there is no outward recognition, except under the most fortunate, and it may almost be said accidental, conditions; if it be carried out in personal form, there is much to bear,—neglect, rivalry, criticism. I would not like to assert that in my professional experience I have seen a life directly snuffed out by these causes, but I have seen minds snuffed out and lives embittered beyond almost the taste of death under severe provocation. I have also seen some injured in their mind's estate, and in their physical health, by the fact that in a moment of thoughtlessness, revenge, anger, or jealousy, they have written what they could not recall, and have lived to realise the terrible fact that what they have written was as false as it was cruel.

These things will, I hope, improve. We are but just emerging out of the savage state in literature, and time will civilise. I have noticed that the feeling of power which the anonymous writer possesses has often a bad effect on his action. He is attacking without being attacked, and when he is in the prime of his manhood, before he has forgotten the gratification of youth in inflicting pains and penalties on his compeers, he feels a false conceit in what he calls "pitching into other men" and laying them as low at least as his own level. There exists at this time a special literature of this kind, contributed by critics of the class indicated. It is a literature which many of the reading public revel in; it is called serene, and, priggish as it may really be, academic, and classical; it leads in the end, amongst those who supply it, to a very bad state of mind and body. Its cultivators stick where they began, run to seed there, and grow wretched as they grow wise.

It is right to prepare the young writer for some troubles in literature; it is equally right to assure him that the pursuit itself is one that may be carried on with as little risk to health both of mind and body as any other profession, and with less risk than attends many. In comparison, for example, with the profession of medicine, it is most favourably placed. The solemn responsibilities of medicine are unknown in literature; the harass, the wear and tear, are unknown. The preparatory and initial stages of introduction to physic, so intensely trying and painful, are also unknown; while the results obtained, if they are crowned with success, are far more agreeable, since in literature the true skill of the writer can be faithfully discovered and followed by all without doubt, whilst in medicine—and the same holds good in politics—it is often impossible to

tell whether the best results are due to art or to nature.

The pursuit of literature is a strengthening pursuit when it is followed with due regard to the requirements for strength. Exercise of the brain is conducive both to mental and physical vitality. Some brain-workers are amongst those who are longest-lived, and happiest in their vocation. Their minds well filled, they have no time to think that harm will befall them, and so they often escape many evils of anxiety and fear which beset other active workers.

The pursuit of literature may be followed by those who are contented with a moderate income. Fortunes may not be won by it, and it may be true that the publisher makes the fortune, the author the competency; but, after all, it is a free life that is uncumbered with the poverty of wealth and the cares by which wealth is earned. The moderately successful author is both free and independent. He can carry his work with him wherever he goes; and if he means to do great work, it is best that he should be removed from all the temptations connected with the luxuries of the rich. "Live low to think high," was the rule laid down by Lord Byron, and a better rule was never formulated. Competency is the order of life for an author, but if he must needs be rich or poor, let him be poor; it is best for his occupation and his reputation.

METHOD IN LITERARY LABOUR.

I pass from these points to the consideration of method in carrying out the profession of literature so that the culture and pursuit of it may be made profitable alike to mind, body, and estate. Most men think of estate first, and of mind and body afterwards; but the student should follow the old saying, and put estate, as it is put in the

prayer-book, last. This is the natural order, for if mind and body be sound, healthy, and active, estate will come. In all my experience I never knew a man with a perfectly sound mind and body fail in the matter of estate, nor can I accept any such idea of failure.

What, then, are the elements of success? Those who win their way in literature must begin their work early in life. Late beginnings lead to slow progressings. It is true that some few men who have turned to letters in their mature years have made fair way; but the numbers of these are few. As a rule, men who begin to court authorship for the first time in their mature years make a mess of it. They have no flexibility of expression, no certainty of style, no clearness of description. If they try faithfully to acquire these desirable qualities, they become old before they have learned them; if they venture to go on without these acquirements, they are simply clumsy workmen, as they would be in any other new calling.

The proper course is to begin to learn to write while the bloom of expression is on the mind; to get over the lesson of what one may fairly call the mechanical art of expression in words; and, as a golden rule, better than all the refinements of grammars, to learn to write in such a manner that what is written can clearly and positively be understood by the writer, understood, that is to say, straight away, without any ambiguity whatever or self-deception. If a writer can do this, he is safe as regards plainness in writing, and very soon will acquire a habit of reading his own composition as if it were that of another person, of reading it to see if it be quite clear, and correcting it where it is obscure so as to be confident of its passing safe into other hands and brains.

And now I am going to give a piece of advice which

some may consider heterodox, but which I dare not in conscience omit. It is that they who determine to follow literature as a profession should under no pretence whatever, under no temptation whatever, begin their lives by entering into any competitive mental strain in order to prove themselves so much more learned than other writers of the same age. I do not bid a student avoid work; on the contrary, I recommend him most earnestly to work so as to become a scholar and an accomplished scholar. I warn him only against striving to become a scholar by violent, pressing competition in scholarship. There is no organ of the body so delicately constructed for work as the brain; it bears the concussion even of knowledge indifferently. It may be worked to an extent that is surprising, filled with fact and charged with imagination to a degree marvellous, but it must have time for the process, or it will be sure to fail prematurely, and if it be rudely trifled with before its work has well begun, it will never fully develop.

Brain pressure must be fatal to those who have to earn their bread by brain-work. Literary workers of all others require to bear gentle and even pressure on the brain. They are men and women to whom life must be a day-by-day new lesson, who must throw newness of argument and freshness of fancy into all they import into the world; they are men and women whose youthful minds must have no surfeit even of luxurious learning, and who must certainly have no store of learning forced on them, no strain and wear of extreme tension on the growing and developing organ of the mind.

In support of this argument history backs science. It tells the story that the brightness of intellectual and literary power has ever come from minds unimpaired by the burdens of scholastic conceit.

DANGERS OF AUTOMATIC SPECIALISM.

It has been constantly repeated by the philosophical historians that different ages present different and, as it were, alternate pictures of mental and physical power. This age was one of genius, that of mediocrity; this of war, that of peace; this of quiet inventive thought, that of quick, sure, successful inventive application and manufacture; this of pleasure, that of laborious anxiety. To the physiologist these phases are all natural phenomena, following from simple causes, as day follows night and night day. The ages of genius wore themselves out by their own efforts; the ages of war, of active mechanical labour, of travel, did the same; then followed the ages of quietude and rest, in which the exhausted energies were recruited, in which the potential of living action was stored up, in which the man universal practically slept and dreamt to rise again in new form, charged with new energy for new efforts, like a man who has risen from a night of sound repose. We are passing through one of these phases, one in which the whole soul of the man universal is quickened to mechanical work, to liberation of all potentiality in regular and systematic order, through work given out and expended by the hands. Every man's hand is becoming mechanical, and there is as much active brain now in some men's hands as there was in the whole organisation of the majority of men who lived a century ago. Such, too, is the force of moral contagion that for the first time in the history of the world women are entering the lists with men, and, no longer content

"To guide the spindle and direct the loom,"

are determined, at least for a time, to share the field of active combat with their sterner partners and to join

with them in the common handicraft struggle for wealth, reputation, fame, name, and honour.

With this mechanical tendency, this desire to cultivate every branch of knowledge on the mechanical principle, so that even mental operations are expected to move with the precision of a steam-engine, there is developed another tendency: that of working in grooves and attaining what is called perfection in special departments of art, science, and literature. To some extent this division is, perhaps, a necessity, but carried into such minuteness as at present exists, it is hurtful beyond expression. The ideal of individual perfection in the performance of the one particular art or craft, without diversity of occupation—that is to say, the devotion of a life to a single intent or purpose—is of all exercises the most ruinous to the vital nervous power. It brings the whole nervous energy into concentration upon special sets of the motor instruments of the body, and before maturity is attained the living man is transformed into an automatic mechanism which becomes so distinctive that no other motion except that which the habit directs is tolerable. Limitation of view, limitation of knowledge, of thought, of sympathy, of muscular movement, becomes a second nature, and leads to dependency of one man on another and others, until the perfect master in his own department is a helpless child so soon as he is out of his own narrow sphere. Dependency breeds dependency, while the splitting up of departments into departments continues without end.

No one who observes carefully can fail to detect that this bane of mechanical specialism is affecting literature, where in all places above others it ought to be stopped. It shows itself in various ways: in the artificial, clumsily constructed work of fiction; in the hard and stereotyped form of review; in the blank descriptive report. If it

be long continued, it will become a new hereditary evil, so that our writers will reproduce writers of specialities, and a new world of limited actors will people the world of literature.

I urge the young writer strongly to avoid this specialising mania. It is destroying all the professions it infests; it fosters pretensions in those who follow it; it gives excuse to the idle to refer every difficult subject to some one else who is master of it; it divides the soul of man into atomic parts, and checks that universal development of intellect which, in literature of all arts, should be the one persistent advancement.

CULLING IN LITERATURE.

In the culture of the literary life it is a wise part always to cull and cull the manuscript before it leaves the hand, and to cull again the proof. Ben Jonson said of Shakespeare how some boasted of their idol that he (Shakespeare) wrote so fluently, he never struck out a line. "I would to God," says loving Ben, "that he had struck out a thousand lines." Ben Jonson was right and honest; it had been well even if Shakespeare had struck out line upon line which he let go forth with his immortal work.

If this were so with Shakespeare, how urgently the rule and the practice of culling applies to other men. The rule and practice amounts, I think, simply to this: to strike out from a composition all that fails to belong to it, be it ever so good. It may not be well to destroy a really good thought; it may be well to retain it for some other effort to which it may be adaptable: but to keep a piece of writing in a composition when it does not fit, because the author is enamoured of it, is great waste of time and waste of skill.

TEMPER IN LITERATURE.

In the culture of the literary life it is essential to govern all selfish passion. The strongest man in the world cannot afford to be angry. Even in the height of imagination, it is unsafe for the writer to let his own passion infuse itself too freely into the passion he would depict. The moment he falls into that mood he falls into it because he is a personality. The true writer is impersonal. He is looking at nature as a portrait-painter is looking at his model, and he is painting nature apart from himself. He may be as grand as nature, as beautiful, for he is copying from her. But when, leaving his model, he writes what he individually feels, then he is listening merely to the impulses of his own heart, gazing merely into a looking-glass at his own features, and recording his own common personalities. In this lies the difference between the writing that is inspired and the writing that is of the earth, earthy, between that which goes forth to be immortal as nature the moment it is written and that which goes forth to be mortal as the man who penned it and destined it to die with him.

This thought leads me to warn the literary scholar who is now beginning his course to avoid such labour as involves even the feeling or sentiment of personal passion. I press on him above all things not to let himself be carried away, as so many young writers are carried away, by the exercise of the critical reviewer's foil. Reviewing of books has become a dangerous game for all young men and women to play at. It leads those who apply themselves to it to think they possess in their hands a power they have a right to use. The belief breeds a conceit which is most hurtful. I doubt very much whether the practice of critical reviewing is a good and fair practice

in any hands. It appears always to me, at all events, to be an art which should be entrusted to none but the most learned and the most judicially-minded as well as learned. Of a certainty it is not an art to be carried out by the inexperienced and youthful, by those whose energies are so commonly many miles ahead of their discretion. I would advise young writers to limit all their labours in the review department to the analytical part of the process. From that they may learn much, for to sum up the details of a book, however indifferent, is to learn some trifle, while to sum up the details of a really good book is always to learn a great deal. Beyond this exercise of the reviewer's skill all is uncertain and unsafe to him in his early days; a direct road to temptation, and often a direct road to ruin in literary adventure.

RECEPTIVE TRUTHS.

In the culture and pursuit of the literary life it is specially necessary to keep the mind open to the reception of all new truths, but yet not receive even the most astounding new truths with the admiration of superstition; if the student errs in that way, he soon finds himself descanting with the admiration of ignorance. The maxims for him to remember in this regard are to curb wonder and to receive the most original expositions of natural fact and law as common parts of natural law and procedure. In this manner he will learn to train his natural mind into proper mood for the reception and for the treatment of new truths with judicial insight.

I have said that literary work is healthy work, and conducive to length of life if the worker wills it so. I wish now to consider how he may let it take this direction.

LITERARY LABOUR AND HEALTH.

To make literary work healthy is a simple process, depending on the mode in which each day is allotted to it, and also on the mode in which the body is physically trained and disposed to carry it out. The first grand point is to begin the day well by rising early.

The universal experience of the wisest men of all ages is in favour of the habit of getting up early in the morning. The practice is closely connected with length of life. It is also closely connected with happiness and activity of life. The physiological facts on this subject are striking. Those who rise early in the morning are, without any doubt, able to work during the succeeding hours for a longer time than those who habitually rise late. So it comes to pass that confirmed early risers usually wake at their ordinary time even when they have gone late to bed, and during the day feel less fatigue than do persons who from habit linger long in bed. There is something in the act of breathing the early morning air which invigorates for the whole day, seems to remove oppressive vapours from the body, and renders all the active organs of the body—the brain, the nerves, the organs of the senses, the muscles, the lungs, and the heart—freer to act and to perform their varied duties.

In addition to these advantages to the physical health from early rising, the mental health is also invigorated and refreshed. In the country no part of the day is so beautiful as the early morning, and in large towns even the same is true. No one has ever seen London so completely as he who has traversed the great city in the first hours of the day. Then its air is clear, its streets are free, all its vastness as a city is comprehended, and its architectural beauties and faults are correctly appreciated.

There is another advantage connected with the practice of rising early at a stated hour. The practice begets the habit of regularity and punctuality. I have never known a man who rose late and at irregular times who was punctual and regular in the after-course of the day. The fixed habit of rising at a given time is in a few years so defined and, as physiologists say, so automatic, that it becomes a pain to break away from the custom. It occurs, indeed, that the sleeper wakes up whether he will or no. In this manner he attains one fixed and habitual method, which prompts him to learn and acquire that method in other acts. He has full confidence he can do one act of the day with precision, and thereby the certainty is impressed on him that he can do more with the same regularity ; he can rely on himself, and very soon those who know him feel that they also can rely on him. Such a man is a strong man, and is sure to get on in the world.

I have only one word more to say on this point. It is that when once awake it is good practice to turn out straight away. The mind is then bright for the day. Second naps in the morning breed heavy, leaden days, in which the head feels bound or compressed, and as if another necessary nap were always coming on. To read in bed in the morning is very bad. To get up briskly is to be sharp and sure till it is time to turn in for another night.

With the early hours of the morning literary work is always most fruitfully associated when the habit of early work is once acquired. Thoughts are freshest then ; the arrangement of thoughts is clearest then ; memory is keenest then. Thus thought, method, and memory conspire together, and labour becomes pleasure. Make the sun your fellow-workman.

In the work of the day of the literary life nothing is

so wholesome as to carry out the work without strain. There is always some little difficulty in getting into harness, but this mastered, work becomes easy enough. Samuel Johnson records that he could always work when he was forced to it; and Miss Martineau tells us that after the first quarter of an hour she found everything easy. With healthy minds this experience is common. There is, however, a danger connected with it that must not be forgotten; that danger is the too long continuance of the labour after it has become easy. With some this danger is serious. The work is so fascinating, the time goes as if hours were minutes, and the physical powers are, as it were, stolen upon and robbed outrageously. I often found myself in this plight in my earlier life, and it is an unsafe plight. It causes feebleness of the senses, irregular nervous distribution, uncertain play of the circulation, and bad sleep.

In working it is wise to have the knowledge of time always in view, to break it at intervals of an hour or so regularly, and to limit it altogether as to duration. I have come to the conclusion that no profitable work can be carried out after six hours of daily steady labour, and I would recommend every student to keep that period in mind as the full period for good and rich mental activity.

I need not say that quietude is an essential part of the literary life, and that the man devoted to the exercise of literary pursuits should have and seek as few distractions of worry and outside tumult as he possibly can. In his leisure he may occupy himself as much as he pleases in other works that do not produce actual weariness, provided that they cease with the performance of them, and interfere nothing with his life's labour when that is in progress.

PHYSICAL CONDITIONS FOR WORK.

There is something to be said as to the condition of the body during the act of writing. It is best to write at a desk which does not permit the chest to rest upon it. It is good to sit upright in writing. It is good to learn to write with moderate slowness. It is essential to write breathing a pure air. It is essential to write in an atmosphere equably warm; the best temperature for literary work being from 60° to 64° on Fahrenheit's scale. Nothing can be more injurious than to work with different parts of the body at different temperatures, with cold feet and heated brow.

The dress should be accommodated to sustain an equable warmth of body, and it should always be loose and easy, so that no undue pressure or band shall interfere with the course of the respiration, the circulation, or the nervous currents. I need hardly enforce in this day that clothing should be clean, be it ever so homely.

The diet of the literary scholar should always be simple and always nutritious. Fish as an animal food is good; other animal flesh taken in any excess is not good. Animal food should be limited to two meals a day, and should give way to vegetable foods, wholemeal wheaten bread, rice, and fresh fruits.

To subdue the taste for high living is a necessity for the culture and pursuit of the literary life. Still more important is it to subdue the taste for strong and stimulating drinks. I say this with the solemn fact before me that nearly all the failures of literary men are brought about by the insane idea that to work well it is necessary to have recourse to some kind of stimulant. In saying this I denounce excessive indulgence in every stimulant, tea and coffee not excluded; but I level the anathema

chiefly at alcohol. That alcohol relaxes the vessels of the brain, allows the heart to throw more blood into them, and for a brief time permits a quicker train of thought, I admit; but all the while it dissipates energy, and at no time supplies power. It deceives as it destroys, and it destroys as it deceives. The strongest men and women living are those who do not take alcohol in any form; and the experience of persons who fairly try abstinence is that more work and better work, whether the work be mental or physical, is performed without alcohol than with it. The general evidence on this point is most conclusive; and, if I might venture to state my own individual experience, I would say that the evidence is as surprising as it is satisfactory. I have worked actively while indulging in a moderate measure of alcohol daily; I have worked actively while abstaining altogether. In a word, I have made direct personal experiment on the subject; and I am bound to state that the work that can be done during entire abstinence is superior in every respect—in respect to amount, in respect to readiness of effort, in respect to quality, in respect to endurance, and in respect to mental ease and happiness—to that which can be done during times of even moderate indulgence in alcohol.

Alcohol does not give strength, does not maintain strength, and its use cannot truthfully be defended on the ground that the body is not strong enough to do without it. When any one feels that he or she requires alcohol to maintain strength, the evidence favours the suspicion that that person is in danger of collapse from the action of the very agent on which reliance is falsely placed.

In the pursuit of the literary life alcohol adds nothing but evil. Exciting the sensual passions, it leads to

wantonness of thought and expression ; exciting to anger, it leads to cruelty of expression ; exciting to haste, it leads to falsity ; exciting to feebleness (and to this it excites most determinately), it leads to fatuity.

How the pages of some of our greatest geniuses in literature are blotted by this curse it would take a lifetime to describe.

I warn the young writer against this enemy of his craft with all the force I can summon to the task. I warn him also against an allied enemy, not truly so mischievous organically, but mischievous in no mean degree ; and that ally of alcohol is tobacco.

A day of letters spent with stages of leisure, with freedom from excess of food, and with entire freedom from stimulants and narcotics is a day of health. It may always be advantageously closed by easy reading, or enjoyment of conversation, or indulgence in artistic pleasures, but to make the close of it quite perfect it should without fail end in going early to bed. To let a warm bath be the prelude to the night's repose is a very wholesome adjunct, but the early sleep is the necessity for the coming day and its duties. In the natural state sleep occurs with the regularity almost of night, ceases with the regularity of the dawn. "It goes with the sun," said one of our old physicians, and he was nearly correct. Sleep, however, is to a certain extent under the control of the will of the sleeper, who during the earlier part of his life can limit it in its duration to five and even to four hours in the course of the twenty-four. He can also so modify the periods of its recurrence that he can, if he will, turn day into night as the time of his rest by sleep. All these departures from nature are injurious. They become, after a while, automatic in their course ; that is to say, by habit the body feels an inclination to sleep and to wake from sleep with a regularity

that is like the mechanical movements of a timepiece, and for a long season of life the artificial habit seems to take the place of the natural. But nature will not be tampered with for ever; and so they who train themselves to take too little sleep, and especially they who take that little in the wrong hours, invariably suffer at last for the mistake, whatever it may be that has led them to cross the boundary-line of natural law. There comes to these persons a period of prolonged weakness and weariness when sleep will steal over them whether it be wished or not; and then, encroaching and encroaching on the waking hours, it hangs at last like a permanent shroud over the whole life, and the sleeper sleeps into actual death, or—and this is the most painful ending—a prolonged sleeplessness extends itself, and the man cannot get natural, wholesome sleep at all. We physicians say of such persons that they suffer from “insomnia.” They do indeed suffer. They fly to opium and other agents that produce sleep, to obtain no more than a temporary relief, and at last they sink into helplessness and into death, remaining to the end sleepless except when under the influence of those artificial remedies, which act on the brain to produce an unconscious state something akin to sleep, but not the true either in character or in usefulness.

The youth who takes great care to lead a very simple and innocent life, and who cultivates sleep so that at the right times it comes over him in an automatic manner, retains throughout his life the almost perfect sleep of childhood. He grows up a happier and, I believe I may say, a healthier man than others. Certainly he lives longer than the rest of the world, and recovers more quickly from the illnesses he may contract through those external influences by which persons become subject to the different diseases that affect mankind. Menander was right when

he taught that natural sleep was the natural cure for all curable diseases.

In this English climate it is always proper for those who are engaged in mental, and specially in literary, work to take at least eight hours of sleep. A strong man in the meridian of life—I mean a man of thirty-five or from that to forty years of age—should not take less than eight hours; a woman of the same age may take nine hours and be all the better for it. Persons who have advanced beyond forty-five years may indulge in a little more than eight or nine hours.

TWO GREAT ENEMIES.

Before concluding this address two other points seem to me to deserve particular attention in connection with the literary life. The first relates to jealousy as the bane of competition, under which the best men are sometimes doomed to fall. I am speaking literally and medically when I affirm from experience and observation that jealousy may turn into actual disease, and may so affect the physical life as to lay the foundation of fatal disease. There is nothing in all callings so opposed to success as jealousy, but in literature it is the danger of dangers. It destroys the quietude of the reasoning soul; it keeps up a fever of the animal organisation; it leads to passions that are as wearing as pain, to competitions as exhausting as the strife of the gaming table, to failures as certain as those which are produced by strong drink. I pray you all who intend to labour in the literary field, do your best to kill all jealousy lurking in your bosoms. It is seated in the centres of the passions, and no doubt in many persons it is strong as life, keen as death. It has often heredity as its root, and so much the more is it hard to conquer;

but it can, by force of will and exercise of reason, be subdued and even utilised for the purpose of honourable ambition, if it be from the first kept in subjection. It is best kept in subjection by the exercise of a resolute determination on the part of the writer to apply the same fair criticisms to his own work as he ought to apply to the works of others, on the grand principle of doing unto others what he would they should do unto him. To this effort should be added the desire to discover in the successful the secret of success. In success there is always a secret, though it be an open one; and in every case there is some opening, which, followed up, leads to success, under which jealousy vanishes, with all its evil train. My advice, therefore, is, go on working and improving your own work; think no evil, feel no contempt, of fellow-workers; find what path in the wide field of literature suits best your powers; and be sure that in the end you will win if you live and make the best of life.

The second point to which it is necessary to refer is the mode in which to take and accept criticism. Literature has never yet been so far advanced in the form of a pure reasoning art as to be anything like perfect in its critical development. There are three kinds of adverse criticism: the careless, the prejudiced, and the indefinite, under each of which every work an author puts forth must come. The careless, where the leaves of the book reviewed are scarcely cut, is the most common; the prejudiced, where usually some personal like or dislike is the ruling spirit, comes next; and the indefinite, in which no opinion worth reading is given, is the most laboured and the least satisfactory. Nothing really need trouble the honest writer under review from either; and if he can only hold on for a time after the criticism has

appeared, nothing does trouble him. Books are the same to authors as children are to mothers: the latest-born is most cherished. The most sensitive man can bear anything that may be said of a work that is five years old. Curiously, too, distance makes difference. A man in London who will go wild over a severe verdict on his work delivered in London will bear a similar verdict from New York with comparative equanimity. I cannot explain the psychology of these curious phenomena; I only, as an old and tried observer, present them as facts, facts which carry this lesson: that in reading criticism the criticised has only one matter to keep in mind: to determine, to make sure, whether anything said adversely is common sense and common justice, and to turn every word of that nature to good account in future efforts.

"For never can true wisdom dwell with those
Who, playing tricks with fortune, dare not look
At their own vices."

To which fine poetical and wise expression I would add one other instruction: that in meeting criticism everything depends on the way in which the ordeal is accepted at the beginning. Taken hardly from the first, it grows harder and harder on each repetition; taken easily from the first, it is never a burden, but rather an incentive to better effort and bolder adventure. And now farewell!

OPUSCULA PRACTICA.

"There are mites in science as well as in charity."

BENJAMIN RUSH.

TREATMENT OF LUPUS WITH SODIUM ETHYLATE.

THE only instance in which I have seen lupus radically cured, if I may use the term "cure," was under the use of sodium ethylate. In introducing this remedy into medicine, I suggested that it promised from its physiological action to be of service in this disease, but I did not suspect it would act anything like so beneficially as it did in the case to which I am about to refer. The patient was the late Count de G. L., a descendant of one of the oldest, perhaps the oldest of the representative, historical families of France. At the time he came to me he was seventy-six years of age. He had passed through a most eventful life, but was still generally healthy and full of vivacity. Four months before his visit to me he had noticed a pimple on the left temple, which gradually increased in extent and became irritable. It went on into ulceration, and then began to show all the usual characteristics of lupus, resisting every mode of treatment so determinately that his old medical attendant, in despair, recommended the sufferer to resign himself to fate. When the patient first came under my observation the ulcer was of the size of a florin, had slightly raised and fairly defined edges, and showed no trace of healing at any

point. The treatment adopted consisted in painting the surface of the ulcer with olive oil and afterwards applying over the oil a thin layer of sodium ethylate solution of the same strength as the solution employed for nævus in the preparation made by Messrs. Robbins from my formula. The oil was employed to lessen the pain, which would have been very severe had the substance been directly applied. As a matter of fact, very little pain was experienced; and after the second application, made two days later than the first, the scale, or scab, which commonly follows the effect of the ethylate, formed well, and produced a hard, close covering over the whole of the ulcerated part. This scab was allowed to remain, but when it broke away at the edges a minim of new solution was dropped on to the broken spot from the point of a newly cut quill pen. In a very few days the edges of the ulcer began to contract, and as the scale became loose it was found that good healing had taken place beneath. In six weeks the whole patch had healed, the skin a little whiter than natural and slightly puckered in fine, radiating fibres, but really exhibiting no deformity nor anything more than a faint superficial scar. Count de G. L. lived for many years without any return of the ulceration, and ultimately died from nervous break-up and old age.

I regret to say I cannot report the same remarkable success of this treatment in other cases in which I resorted to it. In one of these it did not appear to do either good or harm, and the patient still lives; in the other the facts were as follows. The sufferer was sent to me from America, with a large, deep ulcer on the right temple. The symptoms were most definite, and the diseased surface exhibited an oval ulcer full

two inches in diameter. I treated him in precisely the same manner as in the case of Count de G. L., and for several weeks with the same good results. At last the open surface was healed up to a line three quarters of an inch long, an eighth of an inch wide, and about two lines deep. I flattered myself that the healing was practically complete; and as I was leaving town for two or three weeks, I allowed the patient, who had made every preparation to return to America, to make a visit in England to see his native town. While he was away, the little remaining wound began suddenly to bleed rather freely, for which it was temporarily dressed with a styptic pad. A day or two later on, on removing the pad, the bleeding returned profusely, and with that there was also a return of ulceration. Within a fortnight the case was again under my care, and was subjected to the same treatment with the ethylate, but this time without the slightest effect for good; the ulceration extended to the frontal and temporal bones, the cavity of the cranium was laid open, and death occurred, with all the usual terrible results of progressive lupus.

I see, at this moment, no explanation of these different experiences; but, on the principle of the motto at the head of this section of the ASCLEPIAD, I leave them on the record.

One minor point may be noticed here bearing on the best method of applying the ethylate solution. On all previous occasions I had used either a glass rod or a camel's-hair pencil for this purpose. Here I used a freshly cut quill pen. The pen answered so well, I make use of one now always. It is clean, can be cut new each time, carries the fluid conveniently, and allows it to be inserted with much precision.

*DESTRUCTION OF SURGICAL DRESSINGS BY
BENZOATED PARAFFIN.*



ALL surgical dressings should be destroyed immediately after their removal by the one and only purifier, the fire. In every room or ward for the treatment of surgical cases there ought to be a properly constructed furnace with a bright fire from coal or gas, into which the spoiled dressings can be cast and consumed, the products of the combustion being carried away by the chimney shaft. This is not always practical, and in summer-time it may be inconvenient. The difficulty of the fire in the ward or room can always be met, however, by a very simple plan. Into an ordinary glazed iron basin with a lid or cover pour a little common paraffin oil, and as the dressings are removed raise the lid and drop them in, so that they may be charged with the paraffin. Then place the basin containing the dressings in the fire-grate, open the lid of the basin, and setting fire to its contents, allow them to burn until they are all brought to a cinder.

To avoid the odour arising from the paraffin a little *benzoated chloroform* may be added to it. One ounce of chloroform charged to saturation with benzoic acid and added to a pint of the oil is a good mixture. A pleasant odour is diffused from the fluid itself, and unpleasantness is prevented from fumes arising during the final combustion.

In the post-mortem room the mixture is equally useful for the destruction, by fire, of cotton wool, tow, sponges, cloths, or other combustible things that may require to be destroyed. Steel instruments dipped in the oil and then fired are easily and quickly purified before being restored to their case.

ETHER AND CHLOROFORM.

WITH the modified Junker's apparatus described in last ASCLEPIAD some comparative experiments with anhydrous ether and chloroform have been made. With one ounce of ether or of chloroform in the bottle, the temperature of the air being 60° Fahr., the following results were supplied:—

One ounce of fluid in bottle. Temperature of air and ether, 60° Fahr.

	Loss of Ether.	Loss of Tempera- ture.	Loss of Chloro- form.	Loss of Tempera- ture.
The first complete compression of the bellows	30 min.	8° F.	2 min.	2° F.
During the first minute by 5 times $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$, 1, compressions of bellows	120 "	24°	13 "	4°
During the first $1\frac{1}{2}$ minutes	180 "	30°	23 "	6°
" " 2 minutes	240 "	32°	30 "	10°

[were evaporated.

Thus during the first compression 15 times more ether than chloroform

"	"	minute	9	"	"	"	"	"
"	"	$1\frac{1}{2}$ minutes	8	"	"	"	"	"
"	"	2 minutes	8	"	"	"	"	"

It is no difficult matter at all to secure the dryness of the air which is required for correct administration, for it is merely necessary to have a good fire in the operating-room at times when the air is naturally charged with moisture. In operating-rooms of hospitals, George's stove, better known as the "Calorigen," would answer the purpose of a drying stove with precision, and the degree of moisture can at any moment be determined by readings of the dry and wet bulb thermometer. The rule as to this reading should be that whenever the dry and wet bulb differ less than five degrees the air is not sufficiently free of moisture.

This is a very simple rule, and in hospital practice, at all events, could be carried out with regularity and determination.

THE DIAPHANOUS TEST OF DEATH.

IN February last I was summoned by my friend Dr. Edwin Haward to determine whether a lady, somewhat advanced in life, were alive or dead. In the investigation that followed I brought into play all the tests named in my essay on this subject published in *ASCLEPIAD* for 1889 (Vol. VI., p. 13); and, as Dr. Haward has himself reported the facts of this inquiry in the *Lancet* for June 10th ult., I need not repeat here at any length what he has related. I agree with him that it was not wonderful the friends of the lady were uncertain whether her life were extinct, for a more lifelike expression I have never seen in the dead. Moreover, she had some years before been in what was called "a trance," with rigidity of the limbs, from which she had gradually come back to her natural life.

On applying leisurely and carefully the different tests, eight out of ten were found to be in conformity with death; but two did not conform, and one of these might have attested in the opposite direction. The first of these two unsatisfactory tests related to the blood. Blood drawn from a vein in the arm was very dark, but flowed out quite fluid, and did not coagulate when exposed to the air. The blood in the veins of the dead is usually coagulated, and when it is so there is no surer proof of actual death. At the same time, the fact of the blood being fluid was simply neutral in its meaning, the fluidity being nothing more than a sign of change induced by disease. The second of the

unsatisfactory evidences was the important one. It was derived from the diaphanous test. When the hand, with the fingers brought near together, was held before a strong light, there was a distinct red line of coloration between the fingers, as is seen in the healthy living hand, as distinct indeed as it was in our own hands. We were sure from the other evidences that death was absolute, but we let the body remain in a warm room until the final proof of decomposition of the tissues removed every possible doubt.

The lesson to be learned from this case is that the diaphanous test, on which so much reliance has been placed, is of itself positively worthless. I have seen an instance in which a lady who had simply fainted, and who soon recovered, did not show the red coloration during the syncopal state. She therefore might have been considered as dead on the evidence of the diaphanous test, while in Dr. Haward's case the same test would suggest that a person unquestionably dead was still alive. I refer in this place to the particular evidence quoted above in order to impress on the mind the fallacy of the belief, too widely entertained, that the diaphanous test is a sufficient test. It is insufficient; but why it is so I am not ready to explain. Dr. Haward suggests that the circumstance of the continued fluidity of the blood in the dead tissues might explain the difficulty. The hypothesis is ingenious, and I hope in a future number to record a few experiments bearing on the point. Meantime the practical fact that the diaphanous test is untrustworthy remains for remembrance in all cases where tests are required to prove whether a human being is alive or dead.

USEFUL FORMULA FOR FERMENTATIVE DYSPEPSIA.

I HAVE prescribed for many years the following formula for fermentative dyspepsia, and am satisfied as to its value in stomachic cases where there is constantly repeating acid eructation, distension, and what is called "heart-burn."

R Ol Creasoti Pur., ℥xij.; Sp. Tenuoris, ʒjss.; Ammonie Benzoas, ʒij.; Glycerini Puri, ʒvi.; Infus. Caryophilli, ad ʒvi. Fiat mistura. A twelfth part, or half a fluid ounce, to be taken in four ounces of water, as directed.

I usually direct the dose to be taken between meals two or three times a day. If the acidity is considerable a little alkali—potassa is best—can be added with each dose, or as occasion may require.

USEFUL FORMULA FOR SCIATICA.

THE following formula for relieving the pain of sciatica, acute or subacute, is the most effective I have ever prescribed:—

R Pulv. Opii Pur., grs. xij.; Pulv. Ipecac. Ver., grs. xij.; Sodæ Salicylas, ʒjss.; Ext. Cascaræ Liq., Q.S. ℥ Fiant pilulæ duodecim. The pills, one or two to be taken for a dose, as directed.

This pill induces action of the skin, relieves pain, and keeps the bowels free.



Asclepiad No. 2. Vol. 2. Second Quarter 1593.

*y^r most humble servant
John Locke*

*Autotype from an engraving by
J. Boscawen, after a painting by
Sir G. Kneller in Christ Church
Oxford. B.M.*



Portrait of the late General Taylor

My dear friend
John Lusk

I have just received your letter of
the 17th inst. and am glad to hear
of your success in your studies.
Yours truly
J. Lusk

JOHN LOCKE, M.B., F.R.S.

IN the pretty town of Wrington, in Somersetshire, John Locke first saw the light on August 29th, 1632. He was born in a two-storied cottage there, the gable of which was in proximity with the churchyard wall of the place. In the year 1882 I paid a visit to Wrington to see this famous cottage. I found it still intact, and was shown the bedroom in which the birth took place. Recently the cottage has been pulled down, and nothing remains to mark the spot save a stone entablature in the churchyard recording the fact that John Locke was born in an adjoining house. The house in 1632 was occupied by one of the brothers of his mother, Mrs. John Locke.

On October 28th, 1704, in the mansion of Sir Francis Masham at Oates, in Essex, a place about twenty-five miles from London, John Locke died, fully and serenely prepared for his immediate departure from this world, and at peace with it.

The period during which the life of Locke extended embraces seventy-two years. They were, through him, years of mighty birth of thought, years of revolution in the philosophy of the human mind, such as have rarely, if ever, existed from the era of Aristotle himself. The birth of Locke occurred, we may almost say, by accident at Wrington, for that place was not the home

of his father. Mr. John Locke, his father, resided at Pensford, five miles from Bristol, where he practised as an attorney, and was Clerk of the Sewers in Somersetshire, a position different from what he had filled in his earlier life, for although he was bred to the law and acted for a time as clerk to Francis Baber, Esq., a justice of the peace, he had joined the Parliamentary party, and had held a captaincy in the army of the Parliament. His wife, Anne Locke, however, was a native of Wrington, her father, Edmund Keene, being by occupation a tanner there. It is assumed that Mrs. Locke was on her way to visit her parents at Wrington at the moment of the birth of her child, and that she was taken in labour so unexpectedly, she had not time to reach their abode, but putting up at the little cottage above referred to, was delivered of her famous son, who was born and baptised on the same day.

The earliest days of Locke were passed at Pensford, after which he was admitted, under the influence of Colonel Alexander Popham, of Hunt-Street, near Pensford, to Westminster School, from whence he passed to Christ Church, Oxford, graduating as B.A. in 1655; and M.A. in 1658, in the twenty-seventh year of his age.

He remained for some years a student of Christ Church, and continued in the university not particularly well satisfied with the profit of the work usually carried on there in the orthodox system of the schools. He longed for something more, and found what he wanted in the then comparatively new and astounding works of Descartes. He was in this way inducted into science, and was drawn to the pursuit of medical science so earnestly that, although he never graduated as Doctor, he was accepted as a practitioner, and was

often addressed by the title of Doctor. There is no doubt that he cultivated physic for a considerable time, and was esteemed by some of the distinguished of the faculty, by one indeed no less than Thomas Sydenham himself, who was proud to repeat that the method of his own work, "*Observationes Medicæ*," had been approved of "by a person who had examined it from the bottom, Mr. John Locke, who, if we consider his genius and penetrating and exact judgment, or the purity of his morals, has scarce any superior and few equals, if any, living."

Locke was not strictly Dr. Locke, although he understood something of the art of physic, and was applauded by a real master of the art in such high terms; nor did he settle down, in the strict meaning of the words, to practice. In 1664, when Sir Walter Vane was appointed envoy from the English Court to the Elector of Brandenburg, he accompanied the envoy, not in the character of physician, but as secretary, a post to which he was well entitled, owing to the attention he had paid to what may be called philosophical politics or political science. The visit did not last very long; he returned to Oxford within twelve months, and resumed with renewed ardour his philosophical pursuits, his mind more than ever bent towards those studies which bore on natural phenomena.

At this point in the career of John Locke, an event occurred, partly medical, partly political, partly social, which changed largely the whole course of his life in the coming nearly forty years that belonged to him. In 1666 he was brought into contact with Lord Ashley, a philosophical politician and scholar, who later in life, under the title of Earl of Shaftesbury, played a strange part on the stage of the world. Lord Ashley had sus-

tained a fall, and had, his biographers relate, injured his chest so much that an abscess formed in it. For relief he was advised to drink the mineral waters of Astrop Wells, near King's Sutton, in Northamptonshire ; and he wrote, consequently, to a well-known physician at Oxford, Dr. Thomas, asking him to obtain a quantity of the said waters to be taken at Oxford. Thomas at that time was away from home, and he deputed the task to his friend Mr. Locke, who undertook to receive Lord Ashley and have the waters ready on his arrival. Through some error on the part of the person whom Locke had requested to fetch the waters, they were not ready when the patient reached Oxford, and Locke was obliged to wait on him and excuse the delay. That interview between Lord Ashley and Locke sealed at once what may be called their friendship, for it showed an affinity between the two men which reached, in course of time, to close and friendly intimacy. Locke was detained to supper, was engaged for dinner the next day, and later on was invited to go to Sunninghill to stay with his patient. The invitation was accepted in the summer of 1667 ; and some time afterwards, at the request of Lord Ashley, Locke took his case entirely in hand as medical and surgical adviser, with results that will be recorded on a succeeding page.

We have cause to wonder why Locke did not systematically qualify for physic at this date. Intended at first for the Church, he had, after due thought, given up that project, and had allied himself intimately with the profession of physic. He had become acquainted with the Hon. Robert Boyle, had made experiments with the then new instrument the barometer, had taken to the study of chemistry, and had every means for qualification at command. The curriculum was

easy enough. To get the preliminary M.B. it was merely necessary to attend the courses of the Arabic Professor and the Professors of Anatomy and Medicine, followed by a few disquisitions in the medical school. The M.B. obtained, it was merely a question of waiting four years to gain the M.D. How simple ! But Locke refused or declined to pass through these formalities. He did not object to attend lectures, for he attended the lectures on chemistry of Peter Stahl in 1663 ; but he seems to have ignored the professorial instruction for the degree, as if it were imperfect and antiquated, a mere hash of the teaching of Hippocrates and Galen. To qualify him after the time for the regular method had passed, an attempt was made by Lord Clarendon, the Chancellor of the university, to get for him the degree of Doctor of Physic by special grace. Strong as the request was, it was refused by the university, and subsequently would have been refused again had it been pressed, for Locke was not in favour with the ruling body of the university.

However, through the King (Charles II.) he got permission to retain his position as a student at Christ Church, together with all the rights, profits, and emoluments thereunto belonging, without taking holy orders upon him, according to the custom of the college. He also assisted in physic at Oxford, in alliance perhaps, his friend Dr. David Thomas, with whom, as he communicated to Boyle, he entered upon "a new sort of chemistry, that is, extracting money out of the scholars' pockets." This was simple humour. Locke had no need to strive after emoluments of practice. His father, who had died on February 13th, 1663, had left him estates in Somersetshire ; his only brother, Thomas, had died of consumption in the same year, leaving

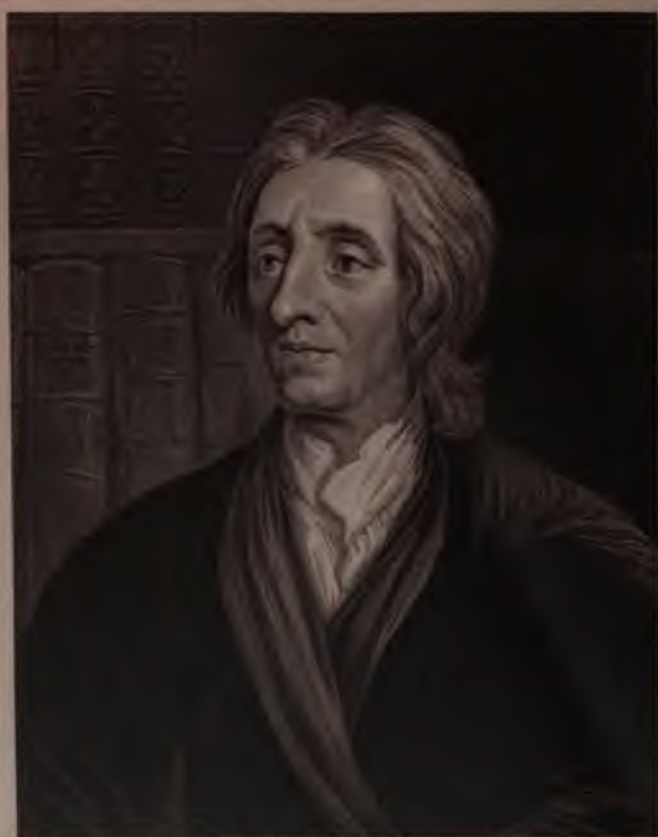
probably to him a further source of income ; and he had the "emoluments," whatever they might be, of his studentship at Christ Church. He was, therefore, a man of independent means—a fact which accounts for his independency of character and of action. It is important also to repeat that, although he would not submit to the orthodox system of entering on a medical career, was, in fact, as we should in this day say, an unqualified practitioner, he retained the friendship and esteem of the best of his medical cotemporaries. David Thomas, Sydenham, Richard Lower, Mapletoft, Willis, not to name others of lesser note, were his good friends. He was strictly at this period an unqualified practitioner, but he was no charlatan. He knew what he was about in physic, and pursued the art with the tact and learning of a practical man as well as a far-seeing philosopher.

THE PRACTITIONER OF PHYSIC.

It has been urged by some scholars that John Locke was never engaged in practical physic, and that at best his connection with the art was nominal. From what has already been told, this theory is shown to be incorrect ; and now the time has arrived when it is in order to indicate how far he was allied to our profession, notwithstanding his rather irregular method of alliance. In his day, although it was not necessary for a man to hold a diploma to practise physic, it was customary for those who were of the higher class to take the university degree in physic and to become connected afterwards with the Royal College of Physicians of London. Locke did neither. He practised far more extensively than supposed. He took part in medic-

obstetric practice, and was obviously devoted to the occupation. To show how this came about we must return to his connection with Lord Ashley.

We have seen that in 1666 Locke made the acquaintance of Lord Ashley at Oxford, and he had been resident at Exeter House, in the Strand, London. In 1667 he was at the same nobleman's country mansion at Wimborne, St. Giles, Dorsetshire, for in November of that year he wrote to Boyle from that house regretting that he had not been able to perform some chemical experiment, although his fingers "itched to be at it." He was now in the position of a medical attendant on the Ashley family, and on the head of it in particular. He came up to London with his patient, and was with him in 1668, when, in May of that year, the said patient's life was despaired of, the abscess, or, as Pepys called it, "the imposthume in his breast," which had troubled him since 1661, having discharged itself into the abdomen. The danger to life was now so imminent that Locke proposed, as a kind of last resource, the plan of making an opening into the abdominal cavity and letting out the offending matter. There was a consultation on this point on June 11th; and, next day, with the concurrence of his colleagues, Locke effected an opening by the use of a caustic, and with such good success that from that time Lord Ashley commenced to recover. In order to keep the wound he had made open, Locke very wisely introduced a tube of silver into it, and so established what in our time we designate drainage of the abscess. Later on another question arose, namely, whether the tube should always be left in the wound or whether it should be withdrawn, and the opening should be allowed to heal up. The subject gave the operator much concern.



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*y^r most humble servant
John Locke*

*Autotype from an engraving by
J. Possewhite, after a painting by
Sir G. Kneller in Christ Church
Oxford. B.M.R.*

the other "*De Arte Medica*." Mr. Fox Bourne, who has published the whole of this last-named fragment, says that the manuscript bears marks of frequent corrections, some of which render it here and there clumsy in expression and almost ungrammatical. "But," he adds, "as it stands it is a capital index to one current of Locke's thoughts when he was thirty-seven years of age." The essay is dated 1669.

It is quite true, as stated by Bourne, that the treatise conveys the current thoughts of its author on the medicine of his own time and of previous times; but it is just to add that neither his learning nor his judgment shines very brilliantly in this effort. The whole, in fact, is an attempt to set up the empirical system of medicine, after the school of Sydenham, as against the experimental system which had so recently been installed by William Harvey and so magnificently advanced by Thomas Willis. The keynote to the argument was written a year previously, 1668, in a paper called "*Anatomica*." There was another hand, that of Sydenham, in this preliminary note, both authors being critical as to the value of anatomical knowledge to the practising physician. Sydenham in his own writing complains of those who "pompously and speciously prosecute the promoting of this art by searching into the bowels of dead and living creatures, as well sound as diseased, to find out the seeds of discharging them." Locke caps this statement by saying that "anatomy is absolutely necessary to a surgeon and to a physician who would direct a surgeon in making an incision, in trepanning, and in several other operations, and that there are other cases in which anatomy is useful, if not necessary, to medical practice. But," he argues, "that anatomy is like to afford any great

improvement to the practice of physick, or assist a man in the finding out and establishing a true method, I have reason to doubt. All that anatomy can do is only to show us the gross and sensible parts of the body, or the vapid and dead juices, all which, after the most diligent search, will be no more able to direct a physician how to cure a disease than how to make a man; for to remedy the defects of a part whose organical constitution and that texture whereby it operates he cannot possibly know is alike hard as to make a part he knows not how is made."

It was in keeping with such an opinion as is expressed above that Locke reasoned on the subject "*De Arte Medica*." It is not possible to say much in favour of this unfinished piece, and it had almost been fortunate if he had destroyed it as an unsatisfactory and crude thesis. His intention, he said, was "to propose some few things to the consideration of the learned men of this so useful a faculty, and to excite their mutual assistance to perfect the art and establish a settled certain practice in the cure of sicknesses, that so, the large catalogue of yet incurable diseases and the frequent sad events of the rest being every day lessened, the diffidence which some sober men upon serious consideration seem to have of the art itself and the disrepute which others industriously labour to bring upon the practice of physick being by the daily growing success of the physicians removed, the industrious and learned practitioners of physick may, with more confidence and satisfaction, attend their calling, when they could no longer be upbraided with their confessed *opprobria medicorum*, which every day yield to the efficacy of their medicines or well-ordered methods."

In a further passage he insists that he who in physick

shall lay down fundamental maxims, and from thence drawing consequences and raising disputes, shall reduce it (medicine) into the regular form of a science, has indeed done something to enlarge the art of talking and perhaps laid a foundation for endless disputes; but if he hopes to bring men by such a system to the knowledge of the infirmities of human bodies, the constitution, nature, signs, changes, and history of diseases, with the safe and discreet way of their cure, takes much what a like course with him that should walk up and down in a thick wood, overgrown with briars and thorns, with a design to take a view and draw a map of the country. These speculative theorisms do as little advantage the physic as food of men, and he that thinks he came to be skilled in diseases by studying the doctrine of the humours, that the notions of obstruction and putrefaction assist him in the cure of fevers, or that by the acquaintance he has with sulphur and mercury he was led into the useful discovery that what medicines and regimen are as certain to kill the latter end of some fevers as they cure in others, may as rationally believe that his cook does his skill in roasting and boiling to his study of the elements, and that his speculations about fire and water have taught him that the same seething liquor that boils the egg hard makes the hen tender."

The opinions thus expressed are characteristic of the mind of Locke; they are metaphysical *versus* physical reflections. For him Bacon, with his experimental system, had lived in vain; for him Harvey had lived almost in vain; for him his illustrious cotemporary Willis, unravelling the mysteries of the nervous system, lived and laboured in vain. He wanted no experimental basis for practical physic; his sole ideal

was of a pure empiricism, as if one should say, "I have treated a set of symptoms called a disease by such and such a method, and finding the method effective, have been content to know no more." It was Sydenham beyond Sydenham, but it was characteristic of the man, and as a line of reasoning it fitted well with metaphysical studies. He contemplated carrying his principles into details; he intended, he says, to treat of—(1) "the present state of the faculty of medicine as it now stands in reference to diseases and their cure; (2) the several degrees and steps whereby it grew to that height it is at present arrived at by experience; by method founded upon philosophy and hypothesis; by botanics, by chemistry, and by anatomy; (3) what may yet further be done, *i.e.*, by what means and method the practice of physic may be brought nearer to perfection."

It might have been a matter of interest, if not of service, to have seen all the designs of this work carried out under the hand of the master. We are not favoured so far. The design is but roughly sketched when it stops short in the unfinished sentence. "Some diseases, like some weeds—" . . . and so ends "*De Arte Medica*," which Fox Bourne thinks was "mainly an exercise for its author, and with no thought, or only a very vague thought, about its future publication." The suggestion is possibly correct; yet it is certain, with Sydenham in evidence, that the ideas sketched out represented very fairly an empirical school of physic, which had its day, and still has it, in an almost exclusive degree. The ideas peer out again in a different style in a preface and dedication which Locke wrote to a work by Sydenham on small-pox, a work that was never published as a distinct or special treatise.

The casual practical life of Locke in the science and art of physic may be said to have closed in 1673-4, but the theory was still to him a matter of great interest. On November 23rd, 1668, he had been elected a Fellow of the Royal Society, on the proposition of Sir Paul McNeil, a friend of Boyle's. He was evidently duly valued in the Society, and very early had offices of trust deputed to him, but he did not seem able to attend to the duties of office, and when he was promoted to the council did not retain his seat there more than a single year at a time. He appears to have had a taste of founding a medical museum, for in 1673 he received from Sir Peter Colleton, from Barbadoes, a jar of country tar, supposed to have qualities of a sanitary character of remarkable virtue, and even to have medical value for the cure of sciatica. With this there was also a pot of tarara root, supposed to be an antidote to the poison which the Indians put on their arrow points, in order to render the wounds made by them fatal.

In 1674, precise date not very clear, Locke became a qualified practitioner by having conferred upon him the degree of M.B. of the university of Oxford. About the same time he obtained two medical studentships at Christ Church. The doctorship might now have quickly followed, but there were enemies still lurking at headquarters, and the doctorate never came to him. By a worse fate he, ultimately, received through the King an order for his dismissal from the studentships, owing to charges of a political character against him, to which reference will have to be made in a succeeding page. He travelled about from this time for many years, visiting France, Holland, and parts of the continent of Europe, making often

medical observations and cultivating medical acquaintances, but only occasionally exercising his medical skill. In Paris he became the friend of Nicolas Thoynard, a great scholar, to whom he wrote giving special instructions about a chemical experiment which confirmed him in the view that in fermentation a new air is generated, and about other learned topics. While in France he also wrote (March, 1677) a letter to Denis Grenville, an old friend in England, on the subject of recreation, suggesting that the best time for recreating the mind is when it feels itself weary and flagging. "The mind," he says, "may be wearied with a thing when it is not weary of it," to which he wisely adds "that the properest recreation of studious, sedentary persons, whose labour is of the thought, is bodily exercise, of those of bustling employment sedentary recreation; that of all bodily exercises those in the open air are best for health; and that it may often be so ordered that one business may be made a recreation to the other."

In 1679 Locke, being again in England, was engaged in consultation at Ollantigh, in Kent, on a Mr. Beavis, whose case he watched and made notes upon with considerable care. During this period he became ill himself, and from September 5th to the 14th treated himself for the cure of a low fever attended with a "subsulting pulse," which became steady as his "stomach and strength came back to him." He speculates here as to whether the recovery was due to "that sympathy between the hand and stomach mentioned by Dr. Godefrois."

In the same year he comments strongly on the nonsense of fashion, the vicious practice, of "strait-lacing." "Narrow chests, short and stinking breath, ill lungs, and crookedness are the natural and almost constant

effects of hard bodice and clothes that pinch. The way of making slender waists and fine shapes serves but the more effectually to spoil them." . . . "'Tis generally known," he adds, "that the women of China, imagining I know not what kind of beauty in it, by bracing and binding them hard from their infancy, have very little feet, whereby the free circulation of the blood is hindered, and the growth and health of the whole body suffers. How much greater inconveniences may we expect when the thorax, wherein is placed the heart and seat of life, is unnaturally compressed and hindered from its due expansion."

The above extract was included in "*Some Thoughts concerning Education*." It indicates the bent of his mind towards the introduction of hygienic principles into practical medicine. The same tendency is shown in his observations respecting the relation of weather to disease, and in his references to the London bills of mortality. The constitution of the autumn of 1679 was, he records in his journal, "intermittent and quarternary." In September there was whooping-cough; in November people had violent coughs without spitting. On November 27th he notes that the mortality for the week in and about London rose to two hundred and twenty-two, an increase scarce ever known out of times of pestilential diseases. The epidemical disease that came in about this time, and caused this mortality, was a dry but violent cough, which produced a "peripneumonia," probably influenza.

Locke himself was a sufferer from some thoracic disease, supposed to be of phthisical nature; and this circumstance naturally led him to the study of phthisis. He delivered the result of his study in a commentary on some observations made by Sydenham, and which he

had confirmed. The most common cause of phthisis he considers to be a cough taken in the winter season. Under the bitter cold preceding the winter solstice, everybody coughs, the transpiration being suddenly checked. So the lungs suffer; and, in extreme cases, extravasated matter is collected up and down in the vesicles of the lungs, which at length have little bags or cystides growing about them, the matter contained in them turning by degrees into pus. "The lungs being thus repleted with pus, from them flow purulent streams into the blood, which cause a sort of putrid fever, whose access is towards night, and its solution towards morning, by a profuse and weakening sweat. Lastly, towards the completing of this tragedy comes on a diarrhoea colliquative, which arises partly from the putrid matter discharged on the bowels by the mesenteric artery and partly from the tone of the bowels being lost and destroyed, and then death is at hand." After describing the hectic fever of phthisis and dwelling on the diagnostic importance of the colliquative sweat, he tells us what still remains true: of the great frequency of the disease in London. He seems to have considered bad air to be the one leading cause of the mortality, and in a kind of inferential manner suggests London coal smoke and its sulphurous products as a possible cause, a theory supported, he thinks, by the statement that in Newcastle "there are more consumptives in proportion to its inhabitants than are almost anywhere else to be found."

On the subject of the treatment of phthisis Locke was once more hygienic, anticipating many of the rules I and others have written on the same subject.*

* See my treatise on "*The Hygienic Treatment of Pulmonary Consumption*" (J. Churchill & Sons, 1856).

His grand remedy was long and persistent riding on horseback in a good air, "which refreshes both the lungs and the blood." Horse exercise does better, he thinks, in phthisis than in any other case, and in his journeys the patient need observe no diet, but may eat and drink what agrees with his appetite; "only let him observe that the linen in which he lies be dry, for the dampness of it will quickly bring back all the mischief."

Fox Bourne states that the memoir from which these passages have been extracted is contained in a curious manuscript volume entitled "*Extracts of Sydenham's Physic-books, and some Good Letters on Various Subjects.*" It was compiled probably in 1685, when the author was living in Holland, or soon after. How far the extracts are merely transcripts from Sydenham's notes and how far they include traces of Locke's own work cannot be easily decided. Other extracts, about which we are in the same dilemma, refer to several important diseases, particularly to diseases of the kidneys, apoplexy, epilepsy in children, delirium, gout, pleurisy, asthma, dropsy, and small-pox.

THE METAPHYSICIAN AND PHILOSOPHER.

It is time to leave "Doctor" Locke—for so he was designated by his friends—to study him in his better-known character as John Locke the philosopher. In his early life at Oxford he had been a lecturer and teacher of Greek, rhetoric, and moral philosophy. At a later period, after his acquaintance with Lord Ashley, he became a writer on various subjects other than medical, as on "*Virtue and Vice: a Utilitarian Scheme of Life,*" on "*Toleration,*" and other kindred subjects, towards which his thoughts were led by Lord Ashley,

who, while appraising him as a professor of physic, thought of him still more as a philosopher.

In 1663 the State of Carolina was presented by Charles II. to eight "lords proprietors"; of these Lord Ashley held the lion's share, and in 1669 an expedition was fitted out for properly colonising the new state. The story of this expedition includes a curious and most interesting bit of English colonial policy; but here I can only note that Locke took part in drawing out the fundamental constitution of Carolina and introduced many suggestions on which the liberty and progress of that constitution rested. In course of time, under the guidance of Ashley, by this date known as Earl of Shaftesbury and Minister of State, Locke became more distinctly political, and held the appointments of Secretary of Presentations to the Council of Trade and Foreign Plantations.

From 1675 to 1679 he passed much of his time on the Continent, pursuing new studies at Montpellier and making friends of philosophers of all classes. In 1679 he resumed work in England, and continued here until after Shaftesbury's so-called conspiracy and death, when, being suspected of implication in some of the treasonable essays of the day, he found it expedient to seek protection in Holland, whither he went in 1684, and was most unjustly expelled from Christ Church, by command of his King, after a connection with the institution of twenty-four years.

The period of his residence in Holland marks the one great epoch in the life of Locke. In the field of medicine he might have taken and sustained a high position had he made the art his chief occupation, overtopped, however, by Sydenham and other of his compeers. In politics he might have made a respectable stand, but his enfeebled

physical powers would have prevented him from taking a commanding place. Philosophy, metaphysics, were in accord with his mental and physical nature. He could think and argue with all the sweet subtlety of his disposition without excess of activity and away from storm and vicissitude. Lord Ashley had detected these capacities and encouraged them in their development; and now they broke forth in the two ever-memorable essays, one on the "CONDUCT OF THE UNDERSTANDING," the other, more comprehensive, entitled "AN ESSAY CONCERNING HUMAN UNDERSTANDING."

It was at one time customary to look upon the first-named of these essays as a kind of rider to the second; it is now more common to take the first-named first, as if it were the prelude of the second. The true position probably lies between these views. The treatise on the conduct of the understanding came secondary to that on human understanding, was, in fact, a kind of exposition—I had almost said an apology—for the larger work, and yet it may have been the summary of the first thoughts of its author in the philosophical line. The opening passage of the work on the "Conduct" conveys this idea, for in a certain and clear sense it breathes the whole breath of the author. "The last resort a man has recourse to, in the conduct of himself, is his understanding; for though we distinguish the faculties of the mind and give the supreme command to the will, as to an agent, yet the truth is, the man who is the agent determines himself to this or that voluntary action upon some precedent knowledge, or appearance of knowledge, in the understanding." The whole philosophy of Locke is concentrated in this one sentence; all the rest is, as it were, an expansion of this root of thought and exposition.

"OF HUMAN UNDERSTANDING."

There are two epistles connected with the work on human understanding which indicate its origin. The first is an epistle dedicatory to Thomas, Earl of Pembroke, bearing date "Dorset Court, May 24th, 1689," the other an epistle to the reader. The first need not concern us, for it is merely after the complimentary fashion of the day; but the second tells us the fact that the essay sprang out of the discussions of a little coterie of friends who were accustomed to meet the bachelor author at his chambers for the purpose of conversation and mutual enlightenment. These friends having a while puzzled themselves without coming any nearer a resolution of the doubts which perplexed them, it came into his thoughts that they took a wrong course, and that it was necessary to see what objects their understandings were or were not fitted to deal with. Some hasty and undigested thoughts on a subject he had never before considered, and which he set down against their next meeting, gave the first entrance into the discourse on the human understanding, and the work thus commenced was continued by entreaty; was written "by incoherent parcels"; and, after long intervals of rest, resumed again, as humour or occasion permitted. At last, in a retirement where an attendance on his health gave him leisure, it was brought to a conclusion. The work, he further tells us, "was the diversion of some of his idle hours, and if the reader had half as much pleasure in reading it as he had in writing it, he" (the reader) "would as little think his money lost, as the author his pains. This is not as a commendation, but simply as a fact."

A short epitome of the treatise was published in 1688,

and met with severe criticism. A first edition of the completed work followed, in which were many faults, owing to bad corrections, explained and apologised for in the second edition. Editions then went on up to the sixth, with alterations and additions; and to this edition the author wrote the epistle to the reader to which I have been making reference.

In the design of the work Locke cast aside at once all considerations of a physical nature bearing on the action of the organs of the mind. He was in this respect the antipodes of Thomas Willis. Willis was all experimental, all physical; Locke was all speculative on sound principles, all metaphysical. He breathed even a spirit of contempt for the physical. "There are speculations which, however curious and entertaining, I shall decline as lying out of my way in the design I am now upon. It shall suffice to my present purpose to consider the discerning faculties of a man as they are employed about the objects they have to do with." In the determination so formed he was probably very wise, since his own turn of thought was foreign—may one say without offence, unequal to the task of striving to understand by experiment and by anatomy the physical principles of mental action; but was it not going somewhat too far to stigmatise such studies, so profound and basic, as "curious and entertaining"?

In the method of his work he pursued the following courses:—

First, he inquired into the original of those ideas, notions, "or whatever else you please to call them," which a man observes and is conscious to himself he has in his mind, and the ways whereby the understanding comes to be furnished with them.

Secondly, he endeavoured to show what knowledge

the understanding has by these ideas and the certainty, evidence, and extent of it.

Thirdly, he made inquiry into the nature and grounds of faith or opinion, whereby he meant that assent which we give to any proposition as true, of whose truth yet we have no certain knowledge, together with reasons and degrees of assent.

Into what debatable ground John Locke entered when he published a book founded on such principles we, in this advanced day, can easily imagine. It would be a startling treatise even at present if it were set free amongst us for the first time. We should wonder, all of us; but some would admire, and many would condemn. The originality of the author as well as his audacity appears in the opening of the second chapter. At the close of the first chapter he takes great pains to define his use of the word "idea," explaining that he uses it "to express whatever is meant by phantasm, notion, species, or whatever it is that the mind can be employed about in thinking." He takes it for granted, as a premise, that there are such ideas in men's minds; every one is conscious of them in himself, and a man's words and actions will satisfy him that they are in others; the first inquiry is how they came into the mind. Presented in this form, the famous second chapter opens with the bold assertion, very bold at the time it was written, that there are no innate ideas in the mind. It had been commonly taken for granted that there are certain principles, both speculative and practical, universally agreed upon by all mankind, which the souls of men receive in their first being, and which they bring into the world with them as necessarily as they do any of their inherent faculties. Any one would easily grant that it would be impertinent to

suppose the idea of colours innate in a creature to whom God has given sight and a power to receive them by the eyes from external objects; it were no less unreasonable to attribute several truths to the impressions of nature and innate character when we may conceive in ourselves faculties fit to attain an easy and certain knowledge of them, as if they were, primitively, imprinted on the mind. The original of all our knowledge is by the impression made on the senses by outward objects; the mind in time comes to reflect on the ideas got by sensation, and thereby stores itself with a new set of ideas, which Locke calls "ideas of reflection." "Thus," to quote the author's own words, "the first capacity of the human intellect is that the mind is fitted to receive the impressions made on it either through the senses by outward objects, or by its own operations when it reflects on them. This is the first step a man makes towards the discovery of anything, and the groundwork whereon to build all those notions which ever he shall have naturally in this world. All those sublime thoughts which tower above the clouds, and reach as high as heaven itself, take their rise and footing here; in all that good extent wherein the mind wanders, in those remote speculations it may seem to be elevated with, it stirs not one jot beyond those ideas which sense or reflection has offered for its contemplation."

If I were to venture on a complete analysis of the treatise on the human understanding, I could give no better view of it than has been rendered in the above passage by Locke himself. The passage is written with great perspicuity, which cannot be said of many other passages. It is also written with a splendour and even poetic beauty, resembling some of the passages

of Sir Thomas Browne, so that it stands like a star brilliant through a gap in clouds sombre and darkening the firmament. Hume and other authors have utilised it in their fashion, but only to rob it of its force and its beauty.

In the admirable chart and analysis of the essay on human understanding, edited by Mr. J. A. St. John, the argument is put in a short and comprehensive form. Taking the word "idea" as comprehending whatsoever is the object of the understanding, the matter of the argument is placed under five heads: (1) Ideas not innate; (2) The origin of ideas; (3) Ideas considered with regard to their objects; (4) Ideas considered with regard to their qualities; (5) Of knowledge, reason, faith, and judgment. This is an able division, and renders the minutest study of the author a comparatively easy task.

Locke started on his great work with the intention of being quite silent on all strictures made upon it. He was, as might be expected, led from the course he had determined on by the criticisms that followed Dr. Stillingfleet, Bishop of Worcester, led to a long epistolary argument, yielding matter for a treatise, which, according to Fox Bourne, is about equal to the dimensions of the original essay, three-fourths of which were contributed by Locke himself. In the end Locke came out the victor. His opponent was able, scholarly, "sly," but he had not the reasoning power of the philosopher. He allowed a sort of spiritual orthodox passion to lead him astray, and, as a matter of fact, raised an indirect attack on some of the heretics of the day rather than on the lay-writer who, in metaphysical form, advanced the new doctrines on the laws of mental action. In his concluding letter Locke, seeing and

feeling the victory he has won, thanks the Bishop in polite, if sarcastic, tone for the pains he has bestowed on the essay which roused his vehemence and his wrath.

“OF THE CONDUCT OF THE UNDERSTANDING.”

The small essay under the above-named title is an important addition to, and, one might say, a companion edition of the more ambitious work. For my own part, I enjoy this essay more than any other of the author's productions, and wish I could follow its instruction as easily as I can admire its construction. The whole is comprised in a hundred and eleven octavo pages, is readable in four or six hours, understandable in four or six days' careful study, and once read and understood, is useful and utilisable for the whole of a long life. There is also this beauty in the teaching, that there is no obscurity in it. It springs from the pure observation of nature. But as, to use a reviewers' common phrase, I would rather the essay should speak for itself to those who have not studied it, I leave it without another word.

THE SOCIAL AND SANITARY REFORMER.

It has been shown incidentally in previous pages that Locke was alive to the social and sanitary requirements of his time. He was far in advance of even later times on these subjects, and a good and practical essay on them, applicable now, might be culled out of his labours. In the service of the State he filled the office for four years and some odd months of Commissioner of Trade and Plantations, and in this office suggested many projects of a social kind that, if carried out, would have had an important after-influence, though whether for good or for evil admits of dispute. He held strong views on

political economy, and rather narrow ones on reciprocity of trade with Ireland. The most interesting of all his labours of a social character is his scheme of poor-law reform in England, a reform which had been lying open for revision from the time of the famous Act of Elizabeth, and which remained in waiting until the first third of the present century. The suggestions made by Locke in his scheme were often very hard and unpractical, but he struck at the roots of many evils, as, for instance, where he observed that "the first step towards the setting of the poor to work ought to be a restraint of their debauchery by a strict execution of the laws provided against it, more particularly by the suppression of superfluous brandy shops and unnecessary ale-houses, especially in country parishes not lying upon great roads." Another point on which he dwelt was education. He suggested that the "guardians of every parish should be able, under a new law, to provide working schools, to which the children of all such as demand relief of the parish, above three and under fourteen years of age, whilst they live at home with their parents and are not otherwise employed for their livelihood, shall be obliged to come." As a sanitarian Locke really takes first rank. He wrote to Dr. Molyneux that "his zeal for the saving of men's lives and preserving their health was infinitely to be preferred to any speculations never so fine in physic." He carried out sanitation personally also as well as by precept. He was neat in attire, active in habit. He was fond of working in the garden, of pedestrian exercises, of horse and of carriage-riding. He was simple in diet, and, like a truly wise man, chose water as his ordinary drink, by which selection he believed he had lengthened his life, although born of a

feeble constitution. He maintained cheerfulness even in illness, and but a short time previous to his death took a lively part in a wedding banquet he himself gave to a bridegroom, his friend King, and his bride, the latter a cousin by relationship. He himself drew out the list of dainties for the feast. "While we are alive let us live," was his motto.

TO THE END.

After passing through various changes of residence, John Locke, to the end a bachelor, yet not without a love-song in his life which never died out, settled down finally in his later years at a mansion called the Manor House at Oates, in Essex. Oates was about a mile from the village of High Laver, about five miles from Harlow, four from Ongar, and six to seven from Epping, a sweetly quiet spot, and one that suited his health—the air pure, the scenery serenely beautiful, the rides and drives enticing. The manor, long destroyed, belonged in Locke's time to a baronet named Sir Francis Masham, an easy-going, happy country squire, twice married, his second wife Damaris Cudworth, the daughter of Dr. Cudworth—one of Locke's early friends,—for whom he felt a sincere affection, and who, in return, felt an equal affection for him.

In 1690 Locke, very ill and wearied of London life, made a visit to Oates, and was so much relieved by the change, he accepted the next year the invitation of Sir Francis and his lady to take up his residence with them, retaining chambers only in town. By arrangement he had his own apartments at the Manor House, for which he contributed his share of expenses without

difficulty—he was worth about £45,000—and found in every sense of the word a home of rest. The record of his life here, as told by Lady Masham and by his own correspondence, is delightful reading. Immortal Isaac Newton, not always as amiable as immortal, was a rather frequent guest, with others whom he chose for association. He lived on in this charmed place until 1704, feeble and latterly deaf, but always in good spirits. He exercised still his medical art on the family around him and on himself, relieving a palpitation to which he was subjected by having taken from him a few ounces of blood. His final illness, leading to death, was lingering. A special carriage was constructed for him, in which he took easy drives, and when this exercise became too tiring, he was carried into the garden in an easy-chair. At a later stage he was borne from room to room. He wrote his last letter to Peter King, the bridegroom of the wedding feast, on October 4th, 1704. He was too weak to write much, but his mind was clear, and he arranged his affairs quietly, leaving the bulk of his fortune to the youngest son of Sir Francis and only son of Lady Masham, who had been as careful to please and observe him as if he had been his own son. So step by step he wiled his way into the eternities about three of the clock on October the 26th, 1704.

By his own request, he was buried, without any trappings to his coffin, on the south side—for he loved the warmth of the sun—of the church at High Laver, where still his remains lie, in a raised brick-walled grave, close to the church wall, surrounded by iron railings and surmounted by a rather large tablet fixed in the wall of the edifice, and bearing beneath his coat-of-arms a Latin epitaph, written, all except the date of

his death, by himself. The epitaph bids the traveller stay : " *Siste viator* "; it tells that near by lies John Locke. If you ask what manner of man he was, the response is, " One contented with his modest fate. Brought up as a scholar, he employed his studies to devote himself to truth alone. This learn from his writings, which will tell what is to be said about him more faithfully than the questionable eulogies of an epitaph. His virtues, if he had any " (*" Virtutis si quas habuit "*), " were too slight for him to offer them for his own credit, or as an example to you. Let his vices be buried with him " (*" Vitia una sepeliantur "*). " Of good life you have an example, would you seek it, in the Gospel ; of vices would there were none for you to seek ! Of mortality, certainly (may you profit by it), you have one here and everywhere. That he was born on August 29th in the year of our Lord 1632, and that he died on October 26th, 1704, this tablet, which itself will quickly perish, is a record."

The tablet has not yet perished. In 1882 I entered the birth-room of Locke at Wrington, a room in the cottage I have described in my opening sentences.* On July 22nd of this year, accompanied by my good friend Mr. Westray, I visited High Laver, and sought for all that could be heard traditionally of our philosopher. So little remained of tradition that our hostess at an inn not far away, who had had a bust of Locke on her staircase for twenty years, did not know the name of the man it represented. The manor-house at Oates was gone, although the site of it was recog-

* I am indebted to the kindness of Mrs. M. Monica Wills, of Barley Wood, Wrington (long the residence of Hannah More), for the information that the cottage in which Locke was born was pulled down recently.

nisable, and almost without the aid of imagination the paths over which the two immortal bachelors, Isaac Newton and John Locke, took their walks and talked their talks were traceable. In the little churchyard the tombs of the Masham family were found falling fast into decay ; but the tomb of Locke was in good repair, and, bathed in the golden light of a setting sun, looked quite fresh and sound. One thing only is wanted, Mr. Gingell, of High Laver, who was so good as to act as our guide, told us, and that is a plate to be affixed to the railings of the tomb supplying plain people who visit the spot an English translation of the epitaph. I promised, if I might have permission, to supply the need.

Meantime to readers of the ASCLEPIAD this tribute to another of the choicest sons of England in the Æsculapian line is herewith presented, with all due respect and affection.

THEORY OF A NERVOUS ETHER.

PART II.

THEORY OF A GASEOUS OR VAPOROUS ATMOSPHERE OF NERVOUS MATTER.

THE hypothesis of the nervous matter being the receptacle of a special nervous fluid, held by the earlier neuro-philosophers, was abandoned, as we have seen, to be succeeded by the hypothesis of the nervous structures charged with electrical force, such force being also liberated by the nervous system. The earlier hypothesis, however, was, and is, as a basis of research, by far the most important hypothesis, but it contained a possible error in this: that its advocates thought the nervous fluid a veritable liquid, which was, as they said, "secreted" by the brain and nerves, as other fluids are secreted by glands. They were led to that view of a fluid naturally enough, because they possessed none of our modern knowledge about organic vapours and compound organic gases having specific boiling-points, specific weights, specific tensions at different temperatures and pressures, and specific powers of conduction of different kinds of force. We are more fortunately circumstanced, for we are conversant with the facts named in relation to an infinite

number of vapours, and we know how living action is modified by the introduction of some vapours into the body. Nay, on the hypothesis of the existence of a gaseous or vaporous atmosphere pervading nervous matter, there are certain descriptions of it which admit of being actually formulated, with the probability that they are near to the truth. We may think of such an atmosphere, for example, as a substance distilled over from blood; as susceptible of condensation under cold; as susceptible of displacement by simple pressure; as soluble in cold blood; as capable of being discharged from portions of the nervous system, and even from the whole under sufficient commotion, and as therefore demanding to be persistently reproduced. We may think of it as holding a precise normal tension at the natural heat of the body; as diffusible readily by heat; as retained longer in cold-blood animals than in warm-blooded animals after death, and longer in warm-blood animals that have died under sudden exposure to cold than in those which have died in heat; as having in itself little cohesion, unless it be condensed round matter which can temporarily retain it; and as having, when condensed in organic matter, the power of conducting electrical vibration, light, heat, mechanical motion. We may think of it as charging the whole nervous system without excess of tension when the natural standard of health is perfect; as allowing other vapours to diffuse through it; as becoming discharged by exercise when the demand for it is greater than the supply; as accumulating in the nervous centres during sleep until the proper tension for motion is acquired, when there is awakening; as present always in life, giving not only capacity for motion, but fulness of form and

tension to the tissues, and as absent in shrunk death, condensed or lost by diffusion.

A spirit, truly, this agent! Yea, truly a spirit, which the ancients thought of in the song and the dream, but which we now foresee as something that may one day pass from the retort into the condenser, to receive a new or retain an old chemical name.

Before I finish this part of my discourse, I would indicate, in a few sentences, how conclusively the theory I have suggested coincides with various best-known phenomena of life and disease, how it correlates, if I may so say. The hypothesis of an absolute fluid matter, a liquid, in the nervous system conveys an idea of grossness of matter incompatible with the refinement of motion peculiar to animal organisms; the hypothesis of an imponderable agency or force minus matter is, on the other hand, like leverage without the lever. But substitute for the two hypotheses the theory of the existence of an organic metallic vapour or gas having a specific weight approaching the weight of the outer atmosphere, distilling over into the nervous matter at every point where blood by its vessels comes into contact with nervous matter, condensable by cold, resolvable by easy chemical decomposition into new forms of organic substance, and the explanation of action is simple beyond expectation. See, for example, how to the motion of the senses there is application of the principle of this theory. When air is put into motion to produce sound, the communication of motion from the air to the tympanum is reproduced in the nervous atmosphere of the auditory tract, and so to the cerebrum. When vibration of ether of space, in wave of light, impinges on the condensing retina, the communication of motion is reproduced in the optic

tract, and so to the cerebrum. When solid particles of matter impinge on the periphery of the olfactory nerve, they excite direct vibration of the nervous atmosphere, and communicate odour. When a mechanical or other impression is made to bear on the periphery or the cord of a nerve away from the senses, the communication of motion again, direct through the nervous atmosphere to the cerebrum, is recognisable as painful or pleasant according to the primitive force of the impression, pain being but excessive motion or vibration of the nervous atmosphere.

The nervous atmosphere, demanding matter and force for its production, becomes to us a source actually of motion. As water from the earth rises into vapour, and returns again in dew, or liquid, or snow, or hail, and by that change makes a circuit of motion, so the nervous vaporous atmosphere, also making constant circuit of motion, is probably, in the foetus, the primary and inductive cause of the after-respiratory actions and of all actions that are involuntary.

The nervous atmosphere, diffused wherever nerve-fibre penetrates, gives the mobility of parts required for motion, saves friction of particles, saves accumulation of force from friction, and equalises.

In the sealed cavities of the cranium and spinal canal, the nervous atmosphere is probably always in tension during waking hours, its pressure being compensated for during hours of repose by the less active cerebro-spinal fluid. In the open parts of the body, in the muscles in the discerning organs, it charges the parts, keeping them in required tension and fitting them for motion on disturbance of equilibrium.

To some extent the nervous atmosphere will be

influenced by variations of pressure, and probably also by variations of condition of the external atmosphere. We are as persistently told this fact by the experience of our daily lives as if we were observing a demonstration of experiment on a barometer. We feel variations of tension as clearly as we can be made to see them, feel them in limb, in joint, in brain, wherever there is nerve-fibre.

I have suggested that, under some conditions of disease, the nervous vapour may be exhausted locally or generally. It may, I think, under other circumstances, be increased. In cases of ganglion of nerve following upon operation, we may assume such increase; in some conditions of brain, it may increase, and on increase produce apoplexy, as from pressure. It may, I can see, accumulate in special centres of nervous structure, and become equalised by what may almost be called an explosion or storm, I mean by a paroxysm of convulsive motion.

The nervous atmosphere, like the outer atmosphere, may be practically poisoned; *i.e.*, it may have diffused through it by laws of simple diffusion other gases or vapours which interfere with its natural function. Thus it may be changed by its exposure to gases and vapours derived from without, and reaching it through the blood, or even directly through nerve; or it may be changed by exposure to gases of decomposition produced by disease in the body itself. It may be influenced by electrical conditions of the external atmosphere; it may even be susceptible of decomposition under great force, such as lightning.

All these, and many other considerations, sweep across the mind when once the idea of an inner nervous atmosphere is simply realised. The theory

harmonises with our sense of observation and practice. Nervous collapse—does it not seem clearer? Effects of pressure on nervous matter—do they not appear less difficult to comprehend? Rapid destruction by subtle organic poisons—is that not explained more easily than by other hypotheses? And the action of remedies—does not the study of that action now expand? Suppose I diffuse into the blood a vapour which prevents chemical change, and stops by exclusion the distillation of the nervous vapour, what are the results? Suppose I introduce a gas or vapour, or liberate one in the body, so that it may diffuse through the nervous atmosphere without arresting chemical force—vapour of chloroform, for instance, or of alcohol—what are the results? Interference with motion; insensibility; anæsthesia. The foreign vapour that has been introduced benumbs; in other words, it interferes with the physical conduction of impressions through what should be the cloudless atmosphere between the outer and the inner existence.

When I first began to consider this question of a nervous ether, I tried to formulate my ideas so as to arrive by a series of physical inductions at some view as to the chemical constitution of the ether or vapour. Thence I passed to experiments in which I endeavoured to discover if by distillation of nervous matter I could drive over and condense a fluid that might be the source of the substance required. Again, by experiment I tried if the injection of certain theoretical fluids could revivify nervous function. Much time and travail has been spent in this exploration, with no direct results, but with one indirect conclusion, or better say inference, namely, that the ether so called is nothing more than vapour of water in certain tension

under animal heat. This I have largely explained in a previous essay on the cerebro-spinal system as a thermal centre and water power (ASCLEPIAD, Vol. VIII., p. 365), to which on an early occasion I must return.

N.B.—In concluding, for the present, this essay, I feel it necessary to add that when the views expressed in it were first published in 1867-8 they did not fail to attract the earnest attention of those mystics who consider the spiritual essence of life as if it were an embodied spirit, existent as such independently of material substance and capable of being existent when separated by dissolution from the material body. It is scarcely necessary for me to say that I had, and have, no intention of advocating that speculation of thought. To my mind the nervous atmosphere is as material as the nervous substance that holds it; and I am more inclined to think with St. James, "For what is your life? It is even as a vapour, that appeareth for a little time and then vanisheth away," than of any embodiment of spirit in ghostly character when the body has fallen into the inertia of death.

COTEMPORARY PRACTICE AND LITERATURE.

"Every physician will and ought to make observations from his own experience ; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together."—FRIEND.

PAROCHIAL SELF-GOVERNMENT.

MR. HENRY C. STEPHENS, M.P., publishes (Longmans & Co.) a compendious and most able treatise on parochial political reform.

The work is preliminary and expository of an important bill, which the author, after a careful study of poor-law administration from its commencement to our day, has constructed for parish government. He divides his subject into five parts. (1) The present position and its difficulties. (2) Concerning departments of local management. (3) What is wanted. (4) A system of complete local self-management establishing one rating authority and one area only under the county. (5) An appendix showing assessments, rates, and meetings of one particular parish—that of Finchley, Middlesex—from 1780 to 1841.

The part entitled "What is Wanted" is that which will interest the greatest number of readers. The scheme here presented is divided into four heads, namely—(a) to establish one local authority only under the county, having power to rate and borrow money for all local purposes upon the property of owners and occupiers ; (b) to establish one area for every purpose of local self-management within the county ; (c) to establish direct

local self-management by the inhabitants in their own local affairs ; (d) to establish supervision and remedial control over rural parishes by their county councils, and to transfer to these certain powers now exercised by the Local Government Board.

The reader will see from this programme what a wide range of subjects is open to him in the study of Mr. Stephens's treatise. It is throughout a most careful and laboured essay, full of admirable criticisms on the past and thoughtful suggestions for the future management of coming populations. It assumes that Englishmen left to themselves, under the guidance of wise and liberal statutes, are fully capable of taking care of their best local interests, without any governance from overworked and incompressive central authorities. It assumes also that it is good for Englishmen to have the local work thrown upon their own hands ; that duties of a local kind are best deputed to local men ; that local rating is educational in its results and calculated to exalt competition into development of the most efficient national industry, strength, wealth, power. The great difficulty Mr. Stephens will have to contend with lies in getting a proper hearing for his design. At present the ruling classes seem all bent on centralisation ; and, strange as it may seem, the classes who are ruled seem equally bent towards the same intent, as if to ease them of all responsibilities were the finest policy that could be organised. The position is critical. The masses cry to the higher powers : " Do this, or we rebel." The higher powers, numerically nowhere, listen, and, for good or for evil, obey. The *vox populi* is their master, be it the voice of God or the devil ; so the masses lose or win by their efforts, as in a game of chance. Mr. Stephens says, Let every man,

and perhaps woman (for he has, I believe, a strong weakness towards the capacity of woman for equal government with man), be a responsible home ruler, and all will go scientifically well and smoothly into a perfect political confederation of labour and peace. Why, he asks, should not the inhabitants have direct control and self-management in the local affairs of their parish? Can it be said that there exist anywhere a better right and a better qualification than they possess? No, it cannot be said. The argument is all on Mr. Stephens's side, and would soon tell if the national mind were educated to grasp the right idea. It were well if Mr. Stephens could see his way to turn his big volume into a simple primer, that all who run might read.

QUAIN'S "*ELEMENTS OF ANATOMY*": *THE SPINAL CORD AND BRAIN.*

PART I. of Vol. III. of Quain's *Elements* has now been published (Longmans & Co.). This part is written by Professor Schäfer, and is illustrated by one hundred and eighty-nine engravings. In so far as my own reading goes on the subject, this history of the anatomy and structure of the nervous system is the best that has ever come into the possession of students of natural science. The medical student who masters this volume may be assured that he has gained a sound knowledge on which he may expend his learning with good effect for many years to come, while the student of general science may be certain that whenever he requires to turn for information to real authority on the brain and nervous system he need go no further than the pages of this volume. It is worthy of note also that there are many points of interest for the general scholar who is in-

terested in biological teaching. Take one instance as a temptation. On pp. 177, 178, he will find a valuable series of comparative measurements of the extent of surface of the cerebral convolutions, and will discover from them that the total surface, exposed and sunken, is about 200,000 square mm. ; that the average thickness of the grey matter has been estimated at 2.5 mm., the specific gravity of the grey matter at 1,033, and of the white at 1,041 ; that the maximum weight of the adult male brain is 64 ounces, the minimum 34 ; the maximum weight of the adult female brain 56 ounces, the minimum 31 ounces. He will also learn the curious fact, to most persons quite novel, that the brain is absolutely heavier between 14 and 20 years of age than at any other period of life ; and that at 80 years of age it has lost rather more than 3 ounces, or about one-fifteenth of its whole weight.

THE LETHAL CHAMBER.

RECORD with much satisfaction the following notes of the lethal chamber from the fifth annual report of the Birmingham Home for Lost and Starving Dogs :—

“Your committee are glad to state that the lethal chamber now in use at the Home gives every satisfaction ; and for the information of those ladies and gentlemen who are not acquainted with the details of the apparatus, a description of it here may not be out of place. It is an air-tight box, in shape like a big dog kennel, built of best seasoned pine. The interior is padded with thick felt, which retains the anæsthetic, and so keeps up always a semi-lethal state. Access to the chamber is obtained from the front by a double set of doors ; the exterior one, which

is hung on strong hinges, and falls back, answers the double purpose of a resting-place for the cage, and when closed makes the compartment additionally air-tight and secure; two other folding-doors open inwards as the cage is pushed in, and swing back again immediately it is withdrawn. The process of charging the chamber is arranged from the top. A metal receptacle receives the fluid, under which a spirit-lamp is placed; this heats the methylated chloroform, the fumes from which descend through two small pipes into the chamber; the chloroform, being the heavier, forces the air out through two escape tubes on the roof, and when the spirit has evaporated, which is proved by a small tap attached to the boiler, the cage, containing perhaps thirty dogs, is rapidly pushed through the folding-doors, the outer one is also immediately closed, and by this time the dogs are unconscious. No sound of distress is heard that can possibly betray symptoms of pain, and their calm repose, when the cage is withdrawn, demonstrates clearly that they have passed away in a final, but painless, sleep. Dr. Richardson, the inventor of this merciful contrivance, says that death by anæsthesia is typically represented in death by chloroform, not by asphyxia, which is typically represented in drowning or in immersion in carbonic acid gas."

"Compared with other methods of extinguishing animal life (such as hanging, drowning, poisoning by prussic acid, shooting, or stunning), the lethal process stands far ahead on every ground of practical readiness, certainty, humanity.

"The objections to poisoning by prussic acid are moral and physical. It is a tax that few men can bear to have every week to take scores of dogs one after another and by force administer to each by the

mouth the fatal dose. Further, the poison is so deadly that a man might easily be killed by a misadventure in administering it."

WHEN MAY SYPHILITICS MARRY?

DR. SCHUSTER, of Aix-la-Chapelle, has written a short but practical essay on this serious question, which Dr. C. Renner has edited and translated into English (Rebman). The question stated depends, generally speaking, on another one: "When is syphilis to be considered as cured?" On these points Schuster has collected and presented the opinions of various competent authors, and, in the end, comes to the conclusion that no syphilitic man should think of marrying for two years, during which time he will have to undergo repeated courses of treatment. "If a person with early relapse of treatment asks me about marriage," he adds, "I reply that he may marry without fear if, after the disappearance of all symptoms, he will take two courses of treatment, with intervals of freedom, for five to ten months; and it makes no difference whether the symptoms occurred in the first, second, or third year after infection."

The author inclines to the view that the foetus can only be infected through the mother, and that the father is unable to transmit the disease to the ovum through intercourse; "he can never procreate a syphilitic child." In proof of this, Colles's observation, ridiculously called "Colles's law," is adduced, namely that an apparently healthy mother is never infected by nursing her syphilitic child at her own breast, not even if the child's mouth be affected with contagious papules, or her nipples be cracked or sore, while a healthy wet-nurse with sore

nipples suckling a syphilitic child acquires syphilitic sores, with consecutive general syphilis; in a word, the mother of a syphilitic child shows immunity from infection. Schuster holds that a patient wishing to marry may present himself affected with tertiary symptoms—that is, gummata—and “in such cases marriage may be permitted as regards power of transmission, as this form is no longer contagious.” Even when a syphilitic person gives evidence of nervous affection and general constitutional disease, if the symptoms be light, he may, after treatment, be allowed in two years to marry. But in cases where later stages of diseases of the brain and spinal cord occur, “after which the faculty of erection may have to be considered,” marriage must be interdicted.

It will be seen from the above that the author before us takes a lenient view of syphilitics in regard to their marriage. He writes with authority; he writes well; and, in so far as the mere mechanism of contagion is concerned, he writes, perhaps, truthfully. But he is too silent about the nervous injuries befalling syphilitics beyond the appearance of specific symptoms; he is also too silent about transmission of evils that lie beyond what is discoverable by outward signs; and in so far he is fallible both as observer and guide.

“*PSYCHOPATHIA SEXUALIS.*”

DR. CHARLES GILBERT CHADDOCK in a volume bearing the above title (F. A. Davis & Co.) has translated a medico-legal study with special reference to “contrary sexual instinct” from the German of DR. VON KRAFFT-EBING, the Professor of Psychiatry and Neurology in the university of Vienna. The translator opens with an

apology, or rather with an apologetical quotation, in which it is admitted that it may be questioned whether it is justifiable to discuss the anomalies of the sexual instincts apart instead of treating of them in their proper place in psychiatry. Attention has also been directed to the baneful influence of works of this kind; and the appearance of seven editions of this present work indicates that it must be largely read out of the sphere of scientific readers. But it is urged, in excuse, that the injury done by implanting knowledge of sexual pathology in the minds of unqualified persons "is not to be compared with the good accomplished." "The courts have constantly to deal with sexual crimes in which the responsibility of the accused comes in question. For the physician exact knowledge of the causes and conditions of the development of sexual aberrations and of the influence on them of hereditary constitution, education, the impressions of every-day life, and modern refined civilisation, is the pre-requisite for a rational prophylaxis of sexual aberrations and for a correct sexual education." Dr. Chaddock, in his preface to the volume, speaks of it as a work conceived and executed in the highest scientific and humane spirit, a work which not only broadens and systematises our knowledge of psycho-sexual phenomena, but also demonstrates, in the results of hypnotic suggestion, how important mental therapeutics must ultimately become in the hands of the physician; a work which is a trustworthy guide in the study of the concrete case of sexual crime, and a philosophical treatise on the inter-relations of sexual criminality, disease, and criminal anthropology.

The praise thus sounded of this work is not undeserved, for the pages are rich in original and most

important observation, opening what is often a *terra incognita* even to the medical reader. At the same time it has many faults, which the translator has not concealed. It introduces a number of cranky and most absurdly unnecessary terms and phrases, and, as if objectionable readings were bettered by learned obscurity of expression, it presents constantly a complexity which worries the mind without enlightening it, and makes the reader feel that he is tormented with a new sort of language which is neither classical nor native, but only pretentious and obscure. The range of subjects is wide; the observation, when it is normally expressed, is remarkable; the judgment, when clearly stated, is sound. The opening section, entitled "Fragment of a Psychology of the Sexual Life," is rich in newness of thought and experience; and, with certain exceptional passages, the after-sections on "Physiology," "General Pathology," "Special Pathology," and "Pathological Sexuality in its Legal Aspects," are all worth attentive study and are calculated to bring to the reader food for reflection, drawn as it were not merely from the volume, but from what, naturally, comes out of his own experience. Some of the principles included are plainly spoken, but to the majority singular at first reading. "Man," we are told, "has a much more intense sexual appetite than woman; but when his longing is satisfied, love temporarily retreats behind other and vital interests. With woman it is quite otherwise. If she be normally developed mentally and well bred, her sexual desire is small; if it were not so, the whole world would become a brothel, and marriage and a family impossible. It is certain that the man that avoids women and the woman that seeks men are abnormal."

Woman, we are taught, is wooed for her favour. Man seeks woman from her physical attraction, and woman, by a kind of instinctive obedience, makes herself physically attractive in response. But women love men, and are drawn to them by the mental rather than the physical qualities. At the same time, the sexual sphere occupies a much larger space in the consciousness of woman than in that of man. The need of love in her is greater than in man, and is continual, not intermittent; but this love is rather more spiritual than sensual.

Readers will gather but a faint idea of this work from what is here related of it. They will, I am sure, like to know more from the book itself, which must, there is no doubt, go quickly into another edition. If it then appear freed of its inartistic German peculiarities; if it be presented shorn of obscure and difficult terms and phrases; if it be printed in one easily read type instead of being here in large type, there in small, on the same page; and, if the reports of cases in small type be all thrust into an appendix, there will be produced a very notable addition to literature, which prudes will possibly condemn, but which wise men will read, mark, learn, and inwardly digest.

“THE LAW OF CREMATION.”

HE little volume entitled *The Law of Cremation*, by Aubrey Richardson, solicitor, is published (Reeves & Turner) as “an outline of the law relating to cremation ancient and modern, together with the rules and regulations of various cremation societies at home and abroad.” The title and description precisely define the object and scope of this work. The book opens with a curious and

most interesting chapter on the ancient laws bearing on cremation, at the close of which it is shown that the influence of religion at length prevailed upon the authorities of the nations of Europe to stamp out the practice of burning the dead. But far back in the prehistoric ages it had been the religious influences of the times that had made cremation an all but universal custom in those communities that first attained to civilisation and to the apprehension of the spirit of true religion.

In the second part of his work the author treats on modern laws in respect to cremation, commencing with foreign law and proceeding to British. He thus brings under notice the legal regulations existing in France, Italy, the German States and Prussia, Denmark, Sweden, Norway, Switzerland, Belgium, Holland, Spain, Portugal, the United States of America, Mexico, Brazil, the Argentine Republic, India, Japan, and New South Wales. Following upon this, our English law comes under notice, together with a full account of the working of the English Cremation Society and the opinion of Mr. Justice Stephen on the subject. The whole is brought to a close with an appendix of special rules and regulations, in detail.

In these days, when cremation is becoming so common a practice, it is necessary for all who favour it to know what is the law relating to it in all civilised communities. The handy volume here named is intended to help such inquirers into correct knowledge, and in this object it is extremely successful. There is not too much said, and yet all that need be said is brought under the eye of the reader in clear and simple language.

"DISEASES AND DEFORMITIES OF THE FŒTUS."

DR. J. W. BALLANTYNE forwards the first volume of a work (Oliver & Boyd) on the diseases and deformities of the fœtus.

About forty years ago I myself ventured an essay on the same subject, for which the prize of the Fothergillian gold medal was awarded me by the Medical Society of London. I have turned, therefore, with much interest to the contribution now before me from the pen of my learned younger brother. I have so far forgotten my own essay—which, by the way, was never published—that I am unable to state at the moment the mode in which the fœtal diseases were there classified, although I devoted very much time and labour to that part of the subject in a synopsis which the adjudicators were specially pleased with; but, turning to Dr. Ballantyne, I find that he classifies under five heads: (1) Idiopathic diseases, or those originating, as far as is at present known, in the fœtus itself. (2) Transmitted diseases, those due to disease in the parents. (3) Traumatic morbid states, the results of injuries received either during pregnancy or in labour. (4) Toxicological conditions resulting from the administration of medicinal or poisonous substances to the mother. (5) Death of the fœtus, and post-mortem appearances in—(a) dissolution, (b) mummification, (c) maceration, and (d) putrefaction. The author is of opinion that the number of idiopathic diseases will gradually grow smaller as more is learnt concerning the effect of maternal and paternal states upon the fœtus, and that the second division of the classification will grow larger at the expense of the first.

A point I do not remember to have seen advanced

previous to Dr. Ballantyne's treatise relates to disease incident to the embryonic as distinct from the foetal state. He confesses that as yet little is known with regard to morbid conditions in the embryo, but that these exist is beyond doubt; and further investigation will probably show that embryonic diseases are more interesting and have a more direct bearing upon the ultimate problems of pathology than have even foetal maladies. What follows upon this observation is of great importance; but as there is another volume in prospect, I shall wait to see that before proceeding further in analysis and commentary on one of the most promising labours the grand old school of Edinburgh has ever given forth.

RURAL HYGIENE.

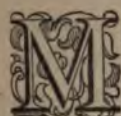
“SSAYS on Rural Hygiene” is the full title of a volume (Longmans & Co.) by Dr. Vivian Poore, an author whose name is widely known in practical medicine and hygiene, and who always writes with the pen of a scholar. There are thirteen chapters between the covers on great subjects, including the concentration of population in cities, the shortcomings of some modern sanitary methods, the living earth, the house, air, water, personal experiences of a country town and of a London suburb, and burial, with a final chapter entitled “The Story of Bremontier.” All these chapters are admirably descriptive, so that the most austere reader cannot speak of them as dry, which is saying a world of deserved praise for any book having sanitation for its theme. The last chapter, a bit of biography, in fact, is quite entrancing. It gives a picture of Nicolas Thomas Bremontier, a sanitary reformer of last century, of whom our English

public knows, I had almost said, nothing, and of whom the French public itself, although he belonged to France, knows but little. Dr. Poore shows in happy language how this man, an engineer, recognising the vegetating power of shifting masses of sand, turned what may be considered a sandy desert, named the Landes, near the Bay of Biscay, into a fruitful, healthy place by planting on the sands the *Pinus maritima*, a species of pinaster, in habit and size very much like the common Scotch fir. The pine seeds were sown mixed with seeds of common broom, and the sowings were made in a direction at right angles to the prevailing wind, a screen of hurdles of gorse or planks being placed to the windward side of the seed-ground. In half a dozen years the brooms had reached their full growth, but the pines continuing to grow, overtopped the brooms, and smothered their nurses. The foresters pruned the pines; and in time Nature, turning them into useful purpose against her own devastation, became a friend instead of an enemy of man. This is a mere taste of Dr. Poore's delightful book; the reader of it will find a feast.

"HAZELL'S ANNUAL."

"**H**AZELL'S Annual" (Hazell, Watson & Viney) for 1893 contains over seventy new articles, and is more comprehensive and useful than ever. To the busy man it has become a daily reference for necessary information, and to the mere curious reader it is a volume the study of which kills time as pleasantly as profitably.

*AN ADDRESS OF THANKS.**



R. PRESIDENT, LADIES AND GENTLEMEN,—

Lord Byron is said to have declared of himself that after the publication of one of his early works he awoke one morning to find himself famous. I dare not venture to repeat this statement as applicable to myself, but I dare venture to say that on the morning when the honour of knighthood which her Majesty has so graciously conferred on me was published I awoke to find myself beloved by my countrymen and country-women beyond any conception that I could possibly have formed of their goodwill and esteem. From that date until now I have been favoured with congratulations: congratulations from friends old and new, rich and poor; from friends long separated and, though not forgotten, long lost; from strangers I have never had the pleasure to meet; from public men and from public bodies with which I have been accustomed to work in unison; and, not a few, from some opposed to my views on public and political questions of moment. To me and mine, personally, these gracious favours are naturally gratifying to an extreme degree; but the

* Address to the Society for the Study of Inebriety on the occasion of a reception and vote of congratulation, July 4th, 1893, Norman Kerr, M.D., F.L.S., President, in the chair.

salt of the gratification, if I may so express it, lies in the one fact that all congratulation is based on the work I have been connected with and have helped to push forward during the past forty years, on the usefulness of that work, and on the assurance that, although I might have been the same if I had not worked at the labours named, I should never have been the recipient of so much reward.

It was fortunate for me that I entered the profession of medicine; that my mother's last wishes, expressed while I was yet a child, were obeyed; and that from my earliest days until now I have been a student of physic, with destined duties before me from which I must never shrink, and from the performance of which I must never hesitate, nor pause, nor tire. For soon in my career I found myself engaged in contests not unworthy of those in which the mailed men of old times strove to win their knightly spurs, and found myself encountering not foes in the armour, but other foes—

“Still as the breeze yet dreadful as the storm”—

in the forms of disease, pain, sorrow, foes quite as fatal, quite as cruel, though less visible and tangible. I joined that corps of brave and earnest men of the past the Owens, the Chadwicks, the Snows, the Grangers, the Milroys, the McWilliams, the FARRS, the Southwood Smiths, the Arnotts, whose whole energies were devoted to the solution of the question, How shall pain, disease, nay death itself as an enemy, be swept from the earth, as vanquished enemies of every race?

That I entered into these contests with enthusiasm I am not afraid to affirm; I entered with perfect freedom and independency of thought and action. I was called by many a visionary; I was often accused of allowing imagination to stand for fact. I heeded

not, but concealing the labour by which I arrived at fact, was content to state what I knew admitted of proof without troubling any one with too many statistics or figures. At the same time I did not despise even imagination founded on observation, but leaving the novelty of the design to make its own way, let it construct for me model cities and communities in which, if the proper plan were followed, health of health would prevail to the full of the possible under the present regime of vitality.

I learned also in course of time the dangers incident to indulgence in intoxicating drinks. I drew a picture of a city I called Toxicopolis, studied carefully the question of perfect temperance on the scientific side, and to the best of my ability taught to the people what I had learned in simple and truthful language, exaggerating nothing and endeavouring to persuade by reason alone. Other efforts for the relief of pain and for removing the pangs of death called for and engaged my attention as well as many more directly connected with the practice of my humane and noble profession; and through all I craved no better reward than what sprang from the fervid hope that at some day, however remote, those efforts, recognised or unrecognised, would be of benefit to humanity. That before I die they should have met with the generous recognition which you, Mr. President, and you, ladies and gentlemen, have joined with so many others in offering me, is beyond all expectation on my part. "I can no other answer make but thanks, and thanks, and ever thanks." I recall from my readings of the past how many men of richer qualities of industry and talent have preceded me on the same lines of research and paths of science, breathing the same aspirations, yet passing away unre-

warded and practically unknown in their day and generation, while I, like a spoiled child of victory, enjoy the fate which had so much more worthily been theirs. The fact would oppress me were it not brightened by the hope that in the better future labours like mine, recognised at last with so much honour, will be so sure of reward that every man and woman following in the same fields of knightly service will feel certain of the same reception, until, under their continued triumphs, this living world shall all be transformed into an earthly haven and heaven of health, happiness, and peace.

TO CORRESPONDENTS.

In order to prevent the frequent and troublesome mistake of addressing communications to the "Editor," it is necessary to repeat that everything published in the ASCLEPIAD is exclusively by the Author whose name is on the title-page.

ERRATA.—*On page 71 of last number, line 11 from the top, and wherever else the word occurs in the article on Erasmus Darwin, for "zoomania" read "zoomonia." At page 47, twelfth line from the bottom, for "drops" read "minims."*

Books, Reports, and Pamphlets intended for notice in the ASCLEPIAD should be addressed to the Author at his residence, 25, Manchester Square, London, W.

Names of Subscribers and other Matters of Business should be forwarded to Messrs. Longmans, Paternoster Row, London, E.C.

Advertisements, and all Communications respecting them, should be forwarded to Mr. A. P. Watt, 2, Paternoster Square, London, E.C.

The Author will be greatly obliged if senders of Newspapers and Magazines would kindly mark the passages to which they wish to direct his attention.

2nd
Quarter,



1893.

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THE MOMENT FOR BLOOD-LETTING.

A STUDY FROM OLD AND NEW PRACTICE.



THE paper on "The Lancet as an Instrument of Precision in Medical Practice" published in last ASCLEPIAD has led, naturally, to many inquiries and criticisms. It has been observed that the paper was wanting in one essential point: it did not supply information as to what circumstances, and during what symptoms, and in what diseases blood should be abstracted from a vein; in a word, what is the right moment for blood-letting. This question is so distinct in itself, it was purposely left out of the previous paper, in order that it might be discussed in another and special form in the present production.

I have as yet seen no rules laid down by any contemporary on this subject. It has occurred within the last few years that some have declared in favour of venesection, or have spoken of it as a neglected practice, or have related an occasional case in which they have felt that good results have followed from it; but no correct indications for general guidance have been formulated. To use the precise words of an accomplished practitioner whom I recently was called to meet, but whom I did not meet, although no time

was lost, until the patient was dead, "This was a case in which I would have liked to have bled, but I was uncertain, as usual, because, although I have been twenty years in active practice, I have seen blood-letting so rarely performed, I do not know the proper moment for the adoption of it as a part of curative skill."

It is not my duty nor presumption to lay down any authoritative rule or rules relating to the difficulty here presented ; but as I have seen two sides of the question in practice, as I have never, during forty-three years of active professional work, given up the use of the lancet, as I have learned to observe without prejudice, and as the scientific side of medicine has always been the favourite side in my work, I may venture to suggest certain rules which have very successfully guided me. They are suggestive simply, not dogmatical.

I saw in my youngest observations some practices of blood-letting by venesection which may be fairly considered as obsolete in this day, obsolete not because they were in their own time worthless—they were, in fact, very valuable—but because they have been superseded by other methods of more recent development. Let me name a few of these, in the first place. Venesection was employed to relieve acute pain, and it was successful. If a patient were suffering from the extreme pain of passing a calculus by the gall-duct or by the ureter, we never for a moment hesitated : we made use of a hot bath, and drew blood *ad deliquium*. The practitioner to whom I was articled thought it excellent practice to bleed when the patient *was in* the bath, but others bled *directly after* the bath, owing to the risk of lifting the patient out whilst he was in a condition of depression from the loss of blood. There is no mistake about it that free venesection in these cases brought rapid relief.

So late as 1865 a retired Indian commissioner to whom I was called while he was passing a gall-stone, and whom I found in a hot bath, begged me to bleed him, because he had on a previous occasion found instant relief from venesection. I bled at once, and with results of the most satisfactory kind. The pain ceased as the sense of faintness came on; the stone passed into the intestine, and was voided on the same day. The mode of action was clear and simple enough: the abstraction of the blood caused relaxation of organic fibre; and the opposition to the obstruction over, the stone passed through the duct, in addition to which the congested liver was relieved. I do not support this practice, though I am not afraid of it, because we have at hand other means in the way of antispasmodics and anæsthetics which answer the purpose and are more in accordance with the opinion of the hour; yet it is worthy of record that this same gentleman whom I so successfully bled, and who had been bled on a previous occasion by another physician for the same disease, died two years afterwards from a similar seizure, during which, in place of venesection, he was treated by antispasmodic and narcotic remedies.

Nature sometimes points out to us that loss of blood is a good mode of relief from acute pain, and the observation of this fact may have led the fathers of physic to adopt the practice of venesection for relief. A patient under my care, suffering extreme abdominal pain, lost suddenly thirty fluid ounces of blood by the bowels. The loss caused great alarm to his friends, and some faintness in the patient; but he was recompensed by the instant relief that followed and the recovery he made, which lasted for several months.

I have recorded in my last paper the reduction of

pain that venesection gave in a pleuritic case, and I could supply other illustrations. One is very curious and striking. The late Mr. James Wardrop, whom I remember as a surgeon of great ability and experience, was so certain of the removal of pain by the free abstraction of blood that before the days of anæsthesia by the inhalation of gases or vapours had arrived he proposed that persons who were about to undergo painful operations should be bled into the unconscious state, in order that the operations should be performed painlessly, and to a considerable extent bloodlessly. He bled into entire unconsciousness a lady who had to be subjected to excision of the breast, and then carried out the operation with complete success. I have myself extracted a tooth from a person in a fainting state without inflicting any pain, and without the patient being conscious that the extraction had been effected. In such examples the mere removal of blood from the sensitive surfaces was the anæsthesia, and we need be no more surprised at free venesection being a relief to acute suffering than at the inhalation of chloroform giving similar immunity. In general principle the two processes are alike: the removal of blood from a sensitive surface deadens it to vibration; the charging of the same surface with an anæsthetic vapour has the same or a similar effect. In these days the common anæsthetic methods put Mr. Wardrop's suggestion out of the field, and also supersede the application of venesection as a special remedy for acute pain arising from irritation or inflammatory lesion; yet there are instances in which there are complications which make venesection still retain its place as a soother, as in examples where pain is produced by the extreme congestion of vascular organs: the

lungs; the liver; the kidneys. I bled a plethoric man quite recently, who, with albumen in his urine, was undergoing what he called "a martyrdom" from peritoneal pain incident on renal congestion. The relief rendered was immediate, and led to the copious emission of urine.

Our predecessors were decisive in their belief that venesection was the safest remedy for the arrest of hæmorrhage. It seems at first sight a contradictory thing to bleed for loss of blood, yet the act was common sense as well as common practice. On the battle-field it would be observed that bleeding from a wound up to exhaustion of blood stopped the current. The heart of the bleeding man grew too weak to pump out the blood from the wound; the column of blood became too small to be of moment; the vessels from which the blood flowed contracted, and the blood in those vessels firmly coagulated and closed up the rupture. These were all conditions favourable to recovery, if the outburst of blood were overwhelmingly rapid at the first. What was thus observed was natural law, and the practice the ancients founded upon it was most reasonable. The observation is good at the present day. In nearly all severe hæmorrhages, the natural reduction of blood through the bleeding surface, the contraction of vessels, and coagulation are frequent causes of safety from death.

In practice we often see patients suffering from hæmoptysis who recover from the attack under the most different treatments. I have noticed this particularly in hospital practice. I have observed that no two physicians treat hæmoptysis, although it occurs in the same form, precisely alike. One orders iced drinks; another pays no attention to such rule, but

prescribes diluted sulphuric or other mineral acid ; a third gives ergot ; a fourth, small doses of acetate of lead or tannin. Each pins his faith to his own remedy because it seems to succeed, and the house physicians and pupils are bewildered at the difference. A single practice may be in common, absolute rest of the body with avoidance of stimulants, and with that the fate of each patient *cæteris paribus* is the same. I have myself followed up every line of treatment suggested, with the modification in several instances of ordering very hot tea instead of ice, or hot tea holding a few grains of tannin, and practically I never could detect a marked difference of result ; there has been in each patient a common result under each line of treatment. In young persons and in acute cases where the lung mischief has been limited and recent there has been a certain degree of exhaustion of a purely physical kind, with a natural and considerable degree of mental anxiety, and gradually the hæmorrhage has ceased day by day, with not unfrequently the declaration on the part of the patient that a sense of relief has been afforded by the loss of blood. I have known also the same to occur when there has been no treatment at all save rest and mild, unstimulating food. These facts leave on the mind the fair inference that in examples where specific remedies were administered they were not the cause of what is often called the cure, but that the outflow of blood stopped the hæmorrhage ; that is to say, a portion of congested lung had been relieved of pressure by the giving way of its minute vessels ; under the loss the heart had become reduced in power, the right side of the heart especially ; a less powerful current of blood had been pumped over from the right to the left heart ; the minute vessels had contracted ;

the blood in them had undergone coagulation ; and the hæmorrhage had been thereby arrested. In short, hæmoptysis had acted the same as venesection, and had played the same part ; it had reduced the pressure of venous blood in the lesser circulation ; it had produced what I have elsewhere designated as syncope of the lesser circulation ; and the symptom that had caused so much anxiety had subsided. If this be just reasoning, the old practice of taking blood in hæmoptysis is logical practice after all, more logical by far than our diverse medicinal treatment by remedies the action of which we cannot physiologically follow if we are candid in our investigation. For how can a styptic administered by the mouth reach the lung so as to constrict and coagulate except through blood-vessels and blood, and why should a soluble styptic, administered and making its way through the blood-vessels and the blood, not expend its constricting influence on the first vessels it meets and on the first blood it meets ? Why should it wait to act on the remote injured parts, or act, specifically, on the blood flowing out of the vessels, and not on the blood flowing through them with which it is commingled ? If we could put a styptic down upon the bleeding lung as we do on an open wound, or if we could direct, by inhalation, a volatile styptic to the bleeding surface, we might at once arrest the escape of blood by coagulation, and (if it were good practice) arrest hæmorrhage from the lung at once ; but that would be quite another method to that of trying to reach the bleeding surface through the blood, an attempt which really carries absurdity on the face of it when it is looked steadily, and without prejudice, in the face.

In early days I bled or saw bleeding done several

times in cases of hæmoptysis, and the effect seemed to me sound, though I did not understand the *modus medendi* as I do now. It seemed to shorten the period of expectoration of blood, a relative observation certainly, but it may be a correct one, and was of a surety that on which our forefathers acted. "Reduce the power of the heart," they said, "and you will reduce the action of the propelling centre; lessen the volume of blood, and you will lessen the tension on the injured vessels, and will favour the process of coagulation in vessels at rest, since nothing favours coagulation like stasis."

The point whether we have improved in these days on the plan of our forefathers, and whether the occurrence of hæmoptysis is not the occasion for drawing blood, is a delicate one. On the negative side we may argue that hæmoptysis is itself the same act performed in a different way; that the result of arrest is the natural cure, and therefore we ought not to interfere, because the natural means is sufficient without the risk of subjecting the patient to an extra loss of blood and to further exhaustion. There is, however, another side to the question. If by a rapid removal of blood we can induce an immediate cessation of hæmorrhage from the lung, we may prevent the exhaustion more readily than by letting the hæmoptysis continue, whilst at the same time we may save cough and prevent a risk I have twice known to occur, asphyxia from the sudden filling of the trachea with blood and coagulation of a column of blood in that tube. On the whole, while defending the ancient plan as strictly scientific and sound, I am not disposed to defend it urgently or to consider the occurrence of hæmoptysis as the necessary occasion for venesection.

As a rule I let hæmoptysis cure itself under rest, with the benefit derivable from the inhalation of spray of tannin dissolved in solution of hydrogen peroxide, and with that I am content, for the moment, to leave the point *sub judice*.

On the same principle that abstraction of blood is a cure for hæmoptysis, our predecessors bled in other hæmorrhages. It may surprise some to hear from me that I once saw a practitioner bleed for uterine hæmorrhage, yet it is the fact; and, what is more to the point, the patient made a first-rate recovery. This was not good practice, although it was in accordance with prevailing ideas at the time. It was not good practice because in uterine hæmorrhage we have the means of applying styptics locally; and it was not good because in the puerperal state the blood, rich in fibrine, easily deposits its fibrine in the heart on sudden and extreme loss of blood. From this cause in a puerperal case I saw a death after blood-letting in the year 1854. The venesection was performed in the middle of the day, and the removal of blood was carried just short of deliquium. So little danger was felt by the practitioner who drew the blood that he did not think it necessary to pay an evening visit. The hæmorrhage from the uterus having ceased, after a little restlessness during the night and desire on the part of the patient to rise from bed, she laid herself down again and fell asleep, in which condition she died. The right side of the heart was found filled with a pure fibrinous concretion which extended into the pulmonary artery, and which bore the impress of the pulmonary valves on its surface.

In other hæmorrhages, especially in those which were slow, the old custom was to stop them by a free

abstraction of blood. This led to the practice of bleeding for dysentery, for bleeding hæmorrhoids, and even for hæmorrhage after extraction of a tooth. That the practice was sometimes successful in these cases is quite certain; I have seen it successful, and I am quite sure that the good effects which sometimes follow the local application of styptics to a bleeding surface are often due to the partial syncope under which the styptics are applied. Great credit once came to me for having arrested hæmorrhage following the extraction of a tooth when every usual means had failed. The fact was that, after some hours of hæmorrhage, I came in when the patient was faint partly from want of food and partly from the escape of blood. From the faintness induced, the hæmorrhage had ceased largely, the pressure was off *a tergo*, the blood had coagulated in the vessels, and the case had become one of syncope rather than of hæmorrhage. I plugged with styptic colloid firmly, and added pressure; but the really useful things done were the acts of feeding, by the œsophageal tube, with warm milk and water, keeping the body for some hours in complete recumbency, and sustaining the animal powers without the folly of stimulation.

While thus indicating that abstraction of blood for chronic or slow hæmorrhage is not a senseless line of treatment, I should not defend it pertinaciously, or suggest that the occasion for the employment of the method is always present when hæmorrhage is the embarrassing symptom, since there are means now at our command of arresting hæmorrhage, means purely local and effective, which our predecessors did not possess. In instances where the hæmorrhagic constitution prevails, either from abnormal fluidity of blood or from abnormal relaxation of vessels, venesection would

add to the danger. In the first of these conditions, that of extreme fluidity of blood, the danger certainly would be increased, since in such cases it would be difficult to arrest the flow of blood from the wound in the vein made by the lancet. In the second condition, where the blood retains its power of coagulation, but the vessels are wanting in tone, it would be a more reasonable practice, though perhaps not the best.

I spoke a moment ago about the effect of loss of blood as tending to cause separation of fibrine and deposition of it in the heart or in the great blood-vessels. I am led, naturally, to dwell for a short time on the value of blood-letting in cases of aneurism. Amongst some it was the common practice to treat aneurism by perfect repose in the recumbent position with repeated abstractions of blood, combined with low diet, a modification of Valsalva's famous treatment. Encouragement was given to the method here named from Lancisi's admirable observation that in large aneurisms the fibrine is deposited in layers within the sac, and that the natural cure consists in the narrowing of the cavity and the strengthening of its walls by the layers of deposit. In cases where an aneurism cannot be tied, as in thoracic and abdominal types of the disease, and in cases where pressure from flexion cannot be brought into play, I am not sure whether rest, small abstractions of blood, and generous diet is not the best treatment that can be followed, for, whatever may be said of it, no better has ever been suggested or practised.

It was once the custom to draw blood in all febrile states, whether the fever arose from contagious or infectious disease or from the presence of what is still known as an inflammatory condition. There was

nothing surprising in this practice, for there is a close alliance between all pyrexias, whatever their nature or origin in the beginning, and there is the common tie that all of them are attended with increment of heat. It was no doubt observed at first that when blood was drawn from the body, whether by accident or by design, the withdrawal in free quantity led to coldness of the body; what more reasonable, then, than to draw blood when there was a high temperature? The temptation became so great that the practice was, at one time, universal. Now it is discarded almost altogether.

The grounds upon which venesection has been abandoned in febrile states are not very satisfactory as a matter of reasoning. There are no statistics of a trustworthy kind which indicate that it was unsuccessful, or that other methods of treatment were or are sounder. I never yet met with a man who on any statistical data ceased to bleed in febrile states, and I do not believe there ever was one who was directly influenced by such data. The fashion for bleeding ceased and by degrees died out much as the fashion of a lady's dress ceases; but this time, perhaps, the cessation, however brought about, was correct, for, when the facts are grasped, abstraction of blood in states of fever or inflammation is not physiological. It is a curious and yet certain truth that abstraction of blood and administration of alcohol have very similar effects on the human body: they both for a time reduce temperature, and they both relax organic muscular fibre, that is to say are antispasmodics, and for this last reason they both, under some circumstances, relieve acute pain depending on spasm. In like manner they do not permanently reduce or sustain the natural animal heat, but both

give a temporary relief, which is followed by a reaction that is or may be worse than the original aberration, because it is fever again flourishing in a debilitated system. I knew a practitioner who thought he had accomplished a very clever thing in a practice of bleeding and at the same time sustaining, as he expressed it, by the administration of wine. To his surprise, the combined remedies helped each other, and he speedily dropped the promising ideal after he had tried to realise it. The patient, who had lost blood, was soon made delirious by alcohol, and though his temperature fell, so did his vital power; his heart did not run so fast, but it faltered, and the coldness induced was lethal, not curative, followed by an acute rallying or reaction, with fever again, which, like that of phthisis, was hectic in its type, and which we should have designated a *synthesis* of hectic if we had seen it brought about by physiological experiment. The truth is that venesection does not conquer febrile heat, except in so far as it reduces, for the moment, the volume of superheated blood. It reduces the fire, but does not reduce the origin of the abnormal combustion. It is like withdrawing some fuel without withdrawing the fire or the air, or the after-supply of blood ready to ignite again and burn in increased intensity. The overaction present in febrile states, either because something too combustible is alight or because oxygen is being liberated or supplied in too large a quantity, is checked, but is not stopped, by venesection, and is succeeded by return of the overaction, a worse evil.

I have witnessed the effect of bleeding in two instances in which I employed venesection under an unusual and fatal temperature: 106° F., in one case,

109° F. in another. In both it failed, but in the example where the temperature was 109°, a case of pyæmia, the phenomena were most marked. The patient was the wife of a medical brother, and I proposed, as an only mode of treatment left open to us, the free abstraction of blood. It was determined upon, and I drew the blood myself. The blood, as it flowed into the basin, raised the thermometer to 109° F.; and the flow of blood was sustained until the pulse fell from 140 per minute to 108, with a tendency to deliquium. The temperature came down to 104° F., and remained so for six hours, with hopes of relief, food, without any stimulants, being supplied. After six hours the temperature again gradually rose until it reached 109° F.; and, as there was no more to be expected from further withdrawal of blood, and as the body did not seem in a condition to risk the loss of more, the disease ran its inevitable course to death, a life burned out by its own fire.

From this evidence alone we may glean the truth that loss of blood does not actually conquer the febrile condition, and may conclude that our predecessors were at fault in their copious bleedings for the pyrexias. There is another reason, too, why they were at fault. They knew as well as any now do that inflammatory blood is rich in fibrine; they recorded the fact that blood on being drawn presents or does not present the buffy coat, and they actually called blood presenting the buffy coat "inflammatory blood"; they discovered fibrine as a blood constituent from the observation of the sizzly or buffed coagulation formed in such blood; they noted the contraction which the sizzly coat sometimes undergoes, and accounted for the gravitation of the blood corpuscles on the ground that the corpuscles fall before

they are entrapped in the meshes of the fibrine; they accounted also for the clear serum which exudes from the clot as being due to contraction of the clot and squeezing out of the fluid with which it had been commingled within the blood-vessels: nay, some, like Lancisi, were of opinion that blood rich in fibrine is not unfrequently precipitated in the heart or great blood-vessels, and that what their predecessors had described as serpents or polypi found in the vessels after death were nothing more than the fibrine in the concrete or coagulated state shaped and moulded to the surrounding structures; but the fact was not known until our time, by the labours of Lehman, that loss of blood favours this same precipitation, and that to bleed when the amount of fibrine circulating in the vessels is largely increased becomes an additional danger of fibrinous obstruction and of death.

It appears to me, therefore, that venesection is not scientific practice in fevers and acute inflammations. The only exception that might be taken to this rule would be in pneumonia in the early stages, in what was called at one time the congestive stage of the disease. In some pneumonias the congestive stage is so acute, there occurs a condition closely bordering on asphyxia, in which the face is suffused and dark, the breathing very oppressed, and the body cool or of natural temperature. In this state a free abstraction of blood may be the best of practices: it may relieve the congested lung, give a freer circulation through the lesser circuit, and create a better oxygenation of blood. These are exceptional cases. When the mischief has progressed so far that new changes of structure have been instituted, with effusion into the cellular surfaces, with vascular embarrassment and with plastic exudation, the with-

drawal of blood would fail to give relief, and would have an actual tendency to increase the consolidation of exuded material.

Our predecessors always bled in apoplexy. By apoplexy they meant certain sudden departures of life, as if the body had been stunned. Apoplexy was to them really a blow, as the term implies. As a consequence, they not unfrequently failed to distinguish the difference between a sudden death from disease arising from the brain and one from failure of the heart. Gradually they recognised different types of apoplexy. There was apoplexy (*a*) with congestion; (*b*) with effusion of blood and clot; (*c*) with effusion of serum; and (*d*) with pallor, in which the usual symptoms of coma, insensibility, and stertorous breathing, were attended with cyanosis of the face and general coldness of the surface instead of the more usual accompaniments of redness and increase of surface heat.

It is perhaps the fact that no clearer descriptions of varieties of apoplexy can be given now than those related above, and if we except the last variety, there is probably no better treatment, when the diagnosis is correct, than venesection in all the forms of apoplexy named. The practice is founded on good and reasonable data. If the apoplexy were truly congestive, and if blood had not anywhere escaped out of the vessels, there could not be a better devised plan of relieving the pressure of blood in the closed cavity of the skull than that of removing blood freely from a vein. We have no medicinal remedy for relieving engorged sinuses; local bleeding, unless it be carried to what amounts practically to general bleeding, could afford no aid; counter-irritation is useless, and purgative action is too uncertain and slow. The free removal of

blood does give an opportunity of relief; and if the temperature of the body be above the natural, and the veins be full or turgid, it can do no injury, while it may do good. If the apoplexy be due to a rupture of an artery on the brain and to effusion, the symptoms being the same, abstraction of blood is still the most rational remedy: its effect will be to check the flow through the bleeding vessel, to remove pressure, and to favour the absorption of serum. Lastly, if the symptoms be due to effusion of serum without escape of blood, we know as yet of no readier way of producing absorption of the fluid effused than abstraction of blood.

A point of great practical importance attends the practice of bleeding in apoplexy. If the remedy is to be of service, there must be no delay in carrying it out. If blood be arrested for a short time in the sinuses and coagulate there, it forms one of the most effective obstructions to return of current and one of the most certain causes of death, whatever treatment be pursued. Probably the delay that is now so commonly practised is the reason why results so different from those recorded by the old school are witnessed in these days. In apoplexy, venesection is the remedy that should not be a *dernier ressort*. Why should it? To a person stricken down, with all the senses obscured, with stertorous and laboured breathing, with hard pulse, tense veins, injected face, heated skin, and immovable or contracted pupil, what injury can possibly be done by taking away as much blood as will relieve the tension? It is no more than letting off the steam from an engine under too severe a pressure, and who of vigorous build can be injured seriously at any time by losing ten to twenty ounces of blood?

Venesection then may be considered as the most rational of remedies when early applied in the treatment of apoplexy. I have resorted to it and seen it resorted to many times, and one of the cases reported on in my last paper (ASCLEPIAD XXXVIII.) affords a striking example in support of it. Candidly it does not always answer, not even in instances that seem best fitted for it; that is my honest experience: but it does answer sometimes immediately and obviously, which is more than can be said of any other single remedy or combination of remedies of a minor and less definite character.

In my last paper I described the value of blood-letting in uræmic coma, and in various communications previous to that one I have dwelt on the same topic. In the convulsive coma of uræmia venesection is the most serviceable remedy I have ever seen, but it is not always of the same value. There was a singularly striking evidence of this last truth in my hospital wards last year. A young woman who was suffering from albuminuria, and who was losing her sight from retinic changes consequent on the albuminous condition, was brought in the first instance under the care of my surgical colleague Dr. Collins. While thus ailing she suddenly became comatose, with convulsions of a severe kind, and was sent to one of my wards. She was bled from the arm, and, almost immediately, was relieved; the convulsions ceased, the coma passed away, and in a few days she was restored to the same state as that she presented when she entered the hospital. There could not have been a better illustration of the direct effects of venesection in the right direction. It was a model case.

At the same time in another ward there lay another

patient, a man about forty years of age, suffering from albuminuria. He had become dropsical; he had fluid in the abdomen, but not in large quantity; and his lower limbs were swollen. Somewhat suddenly he fell into stupor, and soon into deep coma with convulsive twitchings. As in the previous case, and on the same day, I bled this patient, and removed twelve ounces of blood. Blood did not flow freely, and for a little time there was relief; but in the course of the day he died under continuance of the coma. In this last case the conditions for venesection were unfavourable in consequence of the dropsical state. There would be reabsorption of fluid, probably containing urea; but I hoped that by the abstraction congestion of the kidneys might be relieved, followed by freer action of them, and that time would also be gained for the action of purgatives that were at the same time administered. The hope was not realised.

If the experiences recorded in the present paper be taken with those presented in the one in last ASCLEPIAD, there will be afforded a fair view of the circumstances under which I should support the old practice of venesection. It will be gathered that venesection is considered sound practice under several conditions of disease, notably the following :—

- (a) In acute spasmodic seizures, as in spasm of croup, in colic, and in angina with symptoms of oppression from distension of the right side of the heart with blood.
- (b) In acute pain, membranous or spasmodic, as in sudden pleuritic or peritoneal pain or in pain from passage of a calculus, hepatic or renal.
- (c) In acute congestions of vascular organs, as of the lungs or brain, apoplexies.

- (d) In cases of sudden shock or strain, as after a fall or a blow, sunstroke or lightning shock.
- (e) In some exceptional cases of hæmorrhage of an acute kind unattended by pyrexia.

I have been occasionally asked, under what exact condition of a patient may blood be drawn without hesitation or fear of direct danger from the practice? To this question I answer, When the veins are full, and the pulse is firm, regular, full, tense; the pupil natural or contracted; the body at normal heat, or, with brain symptoms, raised in temperature; the bronchi free of fluid; and the sounds of the heart well pronounced.



*SANITATION, RETROSPECTIVE AND
PROSPECTIVE.**

MY first expression in this address is one of deep regret that I am unable to be present personally. There are many reasons why I would have enjoyed speaking to the Association on this spot, two of which are dominant in my mind. In the first place, it was my duty, as President of the Sanitary Inspectors of Great Britain, to have been present, to have played my part in presiding over some of your meetings, and to have been amongst those who were able to accept the gracious hospitality of the chief magistrate and municipal councillors of this great city. In the next place, there are the associations of the city of Glasgow itself. Nearly fifty years ago I was a student here. I was so fortunate as to sit under the instructions of such distinguished men as the late Professors Hannay, Moses Buchanan, Andrew Buchanan, Laurie, Andrew Anderson, Ritchie, Weir, Thomson, Penny, and Easton, all of whom have been gathered to their fathers, but whose memories are retained in my mind with the utmost gratitude and affection.

* Address delivered on the occasion of the meeting of the Sanitary Inspectors' Association of Great Britain at Glasgow on August 17th, 1893, Councillor Crawford in the chair. Read for the author by the City Chamberlain, D. Nicol, Esq.

With sincere regret then at my unavoidable absence, and without further introduction, permit me to make a few observations touching on the past of sanitation, and on the work that lies before us in the continuance of our sanitary labours, with special reference to sanitary inspection.

A REVIEW OF FORTY YEARS.

Let me congratulate the country at large on the great advancement that has been made during the past forty years in practical sanitation. In the year 1855 I started the first *Journal of Public Health*; there were then so few sanitary reformers that all their names were included on one octavo page of that work; now a large volume would not hold their names. Amongst those on the page, 90 per cent. were medical men; now there are almost as many sanitary labourers outside of medicine as within its boundaries. It is curious as well as instructive to read in this day the circumstances under which the *Journal of Public Health* commenced. I may glance for a moment at these circumstances and briefly record what I thought and expressed about the work when it had its origin. I issued the work under the conviction (though many derided the idea) that it was a work of which the nation had great need; I invented for it a motto which has since passed into a proverb, namely NATIONAL HEALTH IS NATIONAL WEALTH; and I conjured up the goddess Hygeia, who, by the way, has ever since been a faithful sanitary companion, as a charming portrait and an introduction to the volume. As an excuse for the *Journal* I used the following arguments. I said that during the previous

ten or fifteen years (*i.e.*, 1838 to 1855) a new fact had opened to the English mind: that not only was national wealth in a great degree dependent on national health, but that this fact, which some less civilised nations had been acquainted with for many long ages past, and which had so lately been rightly understood and valued here, was already accomplishing results which could not, in the end, fail to add greatly to the happiness of our homes, to the health of all classes of the community, and to our prosperity as a great people. Fortunately, also, the simpler and more solid views, which were held by the more important sections of the people, were, I thought, well calculated to encourage a sound and lasting reform in all matters connected with the health and well-being of mankind. Pestilences, which, according to the remnants of an old mythology, were the invincible and corrective agents of an angered Deity, were now seen to be the mere effects of human ignorance, conquerable, and even extinguishable, under the influence of an improved and extended knowledge of natural causes and effects. Legislative proceedings, which were once made up only of parchment documents, diplomatic learning, and some historical knowledge of parliamentary forms, were, at length, becoming tinctured with a notion that science is a reality and has a world-wide mission; and that in the soldier's barrack, the sailor's berth, the artisan's dwelling, the nobleman's mansion, the city, and the town, certain improvements were wanting which no one could appreciate who had not an acquaintance with those physical and universal laws upon which the life and existence of all animated nature depend.

MEDICAL SCIENCE FORTY YEARS AGO.

The theory and practice of the Æsculapian art, that fairest of all human occupations, were passing through a remarkable and important revolution forty years ago. The tendency of medicine, which, a century before, was directed toward the division of diseases into many hundred forms and the formation of the most elaborate and complex nosologies, was being revised, and the whole meaning of modern medical inquiry went to prove that disease is a unity, with a variety of phenomena, and that the causes of disease are reducible to a few elementary forms. The philosophical yearning after fixed principles as to disease was influencing the whole system of medicine. If the elements of diseased action were few and simple, the principles of prevention or cure were, it was thought, few and simple also. The "materia medica" was undergoing a thorough revision and curtailment. The viper no longer yielded its body to the formation of a "specific" medicinal broth; the philosophic "oil of bricks" had become a droll myth, despite its magnificent name and former great reputation; a smell of the blooming dog-rose was held to be as remedial a proceeding as the deepest draught of an infusion of its lifeless leaves; and these, and a thousand other *remedia admirabilia*, were gone or were going into the catalogue of the things that had been. Meanwhile a new pharmacopœia had come into sight, which all could read. Its principles were preventive, its objects wide, and its elements—some seven only, and the world's general property—were no more nor less than pure air, proper nourishment, a regulated temperature, bodily exercise, cleanliness, mental education, good morals. Thus, in some of the

most important sections of the community, there was a general improvement and simplification of knowledge of great and vital questions upon the correct solution of which so much of the world's happiness and progress rested.

Many events about the time named tended to assist in effecting the great changes that have since occurred. Nothing helped more in this direction than the lesson of the Crimean campaign. That campaign taught, almost universally, that from a spirit either of ignorance or indifference arose the great living sacrifice of the brave men who died unhurt by the fire of the enemy; and this fact, coupled with the attack of the cholera plague, urged the people to consider seriously the meaning of health and disease, and to listen to the useful sanitary lessons which had been set forth by a few earnest men, without much success.

MEDICAL OFFICERS OF HEALTH.

A little later on a further change took place in England, especially under the reform in municipal government, introduced largely by the late Sir Benjamin Hall (Lord Llanover) in the Metropolis Local Government Act. It is not possible to say that this was the best Act which could have been brought out, but it was not without great merit. It removed many old and useless measures long carried out, piecemeal, by the vestries; and, above all things, in course of time, it led to the appointment of the medical officers of health, who very soon exercised a powerful influence. Let me, however, say at once that, in my opinion, the formation of the medical officers of health was from the first unsound, and very inferior in some respects to the American system of

official medical supervision. It was founded on a system natural enough to the English mind: that of election after the usual canvass of the parochial authorities, but it placed the medical officer in an inferior position. It made him too absolutely the servant, in the most ordinary sense, of the elective body; his salary was constantly most inadequate; his independence was imperfect; in many instances those in authority over him were the chiefest of sanitary sinners, and it was with the greatest temerity he could carry out, strictly, the duties assigned to him; and, not unfrequently, he was allowed to practise the curative as well as the preventive part of his profession, a plan which often paralysed his efforts. To some extent these evils are present up to this hour; but, taking it all in all, the work of the medical officers of health has been stupendous in its useful consequences. The reports of these officers since 1857 would fill a large library, and the historian of the future will find nowhere surer indications of the progress and intelligence of the nation than in these remarkable documents. I trust that some public library will collect the whole of them, so as to place them at the command of the writers of the coming century. It is not always the longest of these reports that is the choicest, although I do not think there is one that is inordinately long, but it is the suggestive spirit, which even in short reports is so frequently present, that constitutes their real value.

THE ORIGIN OF THE SANITARY INSPECTOR.

It was soon found by experience that the medical officer of health required a working hand, since it was impossible for him to go from his office to inspect

every danger to the public health. In this way there sprang up the sanitary inspector. Nothing could be simpler, if I might say so, than his origin. The medical officer of health wanted some one who would not be afraid to inspect any nuisance or to go into any place where there was fever or other contagious disorder. It was necessary that he should be, what was called, a respectable man, and he had to be to some extent presentable in private houses. His duties were laborious, his salary contemptible; I designated him in his first days as the "forlorn hope of sanitation," which my colleague, Mr. (afterwards Sir Edwin) Chadwick, thought was a very happy description.

Gradually the sanitary inspector, from being a "forlorn hope," became an important officer; he took the outside work of sanitation. He began to make reports to his board on elementary points, and saved the medical officer much minute trouble. In a further stage he learned the art of disinfection, and studied the best means for removing the sick from their own abodes to the different hospitals. He also became what might be called a propagandist, teaching practically the rudimentary principles of health to the masses. At last, by the natural method of improvement and progression, he became an order in the sanitary circle, and ten years ago raised himself into a distinct organisation or society, which has gone on improving until the present hour.

It was fortunate for the Society that at its inception it gained the adhesion of one of the most powerful men who ever lent assistance to the progress of sanitation, and whose long life was a continued service to the sanitary cause. I refer to Sir Edwin Chadwick. Chadwick, whose friendship I claimed for more than thirty years, seemed to me to have but one object in view: the

health of mankind. I have designated the two volumes, epitomising his chief works, *The Health of Nations*, and I do not think anything fairer could be said of them. He was a man of entire independency, for, although he had nominally been called to the bar, he practically belonged to no profession, and was free of any kind whatever of professional prejudice. He was an individualist of the purest order, and imperial in his method. In a word, he was the very man to take the leadership of a new and independent working body of sanitarians, and the devotion he paid to his duties was in accord with his life. All who served under him, and are now present, will, I know, support me in this tribute to his memory, which I have simply felt it a duty to point to as a mark of affectionate respect and a tribute never to be forgotten.

GROWTH OF THE SANITARY METHOD.

By the different steps of progress indicated above, sanitation has now become the leading subject of the day. For many years it seemed to me a hard battle, a kind of presentment of details, which required a great deal of study to digest, and were not altogether pleasant when digested. The Registrar-General, who, under the genius of the late Dr. William Farr, had commenced so well in England (backed up by the Registrars-General of Scotland and Ireland), did splendid work in formulating the phenomena of life, disease, and death; and in Ireland, on the suggestion of the late Sir William Wilde, the State document called *The Census of Disease in Ireland* contributed immensely to our knowledge. Indeed, the returns of the registrars, originating in the fertile mind of Chadwick, has formed the capital of our vital statistical information. Still all was considered

heavy and dry by the people, even amongst the most educated classes. An educated lady who read one of my papers returned it saying, "It is a dry book," and explained that it was like roast pheasant with nothing more succulent than ground toast.

This set me thinking whether a little imagination might not be infused into the subject; and so, the opportunity occurring in 1875, I ventured with much fear and trembling to depict to a large audience of sanitarians, in the Dome at Brighton, the imaginary city of Hygeia. It was, in fact, a romance of science, and it might have ended in dismal failure. For some happy reason it went all the other way: it took with the audience to whom it was delivered; it took with the press; and, in a few months, it was all over the world. Some wrote to me for the latitude and longitude of Hygeia; others wanted to know the prices of houses and lodgings; a speculative company was endeavoured to be formed to found the city on the English coast; another company was formed for the same purpose for Florida; a royal potentate had the essay printed in full in the official organ; an aged queen would have it read to her in sections daily; an enterprising print-seller did me the honour to have *Hygeia* bound up for a penny with Bunyan's *Pilgrim's Progress*; and Hygeian residences, boots, shoes, trousers, corsets, braces, with other articles of dress, and even articles of food, became quite common. I have often been amused with the results of this journey into the land of dreams, but have never regretted the excursion, since it popularised sanitation, and did not at the same time propose a single thing that could not be done. That, in brief, was the secret of the success. I felt my way with every step, so that nothing should be suggested that

could not be carried out. I never intended to suggest an actual city of Hygeia, but I wanted the principles advocated to appear everywhere as they have done.

In this brief retrospect I am unable to enter into further details of sanitary progress ; suffice it that the sanitary movement is now the topic of the day. The press is full of it ; parliamentary debaters are anxious to discuss it ; the medical profession, in its usual spirit of true nobility, forwards it, albeit its success is in direct opposition to pecuniary interests of a professional kind ; the clergy, becoming charged with the idea that it is their mission to work and preach in so good a cause, have formed for themselves a Church of England Sanitary Society ; literary and scientific institutions are throwing open their lecture-rooms freely for the use of the sanitary teacher ; the board schools are elementary training schools ; legatees are leaving money for sanitary purposes ; and there are as many sanitary societies at work as there were sanitary individuals fifty years ago.

These are all hopeful and refreshing signs, and if the popular feeling in behalf of the health movement be but judiciously cultivated the results will become in the end of an importance which few at this moment realise. They will lead to what may be called a new race ; the prophecies will be fulfilled, "There shall be no more thence an infant of days nor an old man that hath not filled his days." "The last enemy to be destroyed is death." Death, too, will be euthanasia. Nature, with her foot on the cradle, will put men and women into the final sleep with so much unconsciousness that they will know no more of their death than of their birth.

But intended reforms, to be safe, must be gradual, must be founded on actual knowledge, and must be understood

by the masses in the order they are set forth. This statement, which holds good in a general sense, is specially true in relation to sanitary inquiries; for so intimately is each step in the art of preserving health mixed up with social questions of a sanitary kind that a few false moves or impracticable and perhaps unscientific attempts, hastily made by amateur sanitarians for the mere purpose of doing something, may do harm ultimately instead of doing good to the cause they are intended to support. Thus the efforts that are being made by Parliament and by many public bodies are too often put forward without regard to system, and without any definite or comprehensive object. The elements of the sanitary question are few in number, but they lie deeper than is generally supposed, and many notable health measures, too temporary in their character, are scarcely calculated to affect the great question at issue. There is a strong feeling abroad at this moment that legislative enactments are the most capable of doing service in the preservation of health and in the suppression of disease. I would not deny that a decision in a law-court may check or remove some real cause of disease; but I doubt altogether the correctness of extreme principles of coercion or compulsion. I doubt the general competency of the men upon whom devolves the duty of inflicting fines and penalties on those adjudged guilty of breaking laws of health. It seems to me that if a man understands natural laws of health he is not likely to break them, and I am sure that no man can be a judge who does not understand those natural laws; therefore a judge must be a sanitarian, specially selected, which is not yet a probable, I had almost said not a possible, selection.

THE ONWARD MOVEMENT—A FOURFOLD TASK.

The labours of the true sanitary reformer lie in four directions :—

1. In an endeavour to understand simply the nature of diseases, their alliances, their true distinctive characters, the modifications to which the body is subjected under the influence of diseased action, and the chemical or physical measures best adapted for removal and prevention of disease. Labours of this kind require the highest order of medical intellect; they are altogether removed from the business of the mere politician; and they imply in him who prosecutes them a mind fully charged with all the modern doctrines of chemistry, physics, and the laws of life. These labours tend to lead the mind from effects back to causes.

2. In an endeavour to seek out primarily the causes of diseases, irrespective of the symptoms and the other details involved in the consideration of the diseases themselves. Efforts in this line of research should embrace observations conducted on a large scale, and bearing on the effects of locality, climate, season, meteorology, contagion, habit, diet, and occupation, in so far as they give rise to distinctive types of disease.

3. In striving to make the vast stores of information already acquired in regard to the two forms of inquiry above noted accessible to all classes of society; and in having them scientifically popularised and diligently taught, especially amongst the child population.

4. In giving free scope and encouragement to those mechanical arts which tend to improve the beauty and convenience of towns and cities; in lessening muscular labour; in increasing the comforts of the poor man's home; and in instituting such an elevated class of

amusements and occupations for leisure moments as shall make the heart happy and the mind healthy.

In these offerings to reason and knowledge lies the one and true *elixir vitæ*, the beginning and the end of sanitary science.

Principles ignored, all else is practically mere diletanteism, mere playing with details of principles, the principles themselves being undeveloped—a common error. Right honourable and honourable gentlemen may debate in the senate-house, may sit on committees of inquiry, publish tons weight of blue-books, and invest magistrates with new powers; but the mortality tables of the Registrars-General will continue to tell how little can be done until a new educational impulse is given to the people, leading to personal health, to health of the whole community, springing from health in the unit of that community.

There are some diseases that stand forth independently altogether of what are usually understood as communicable influences, diseases which have a specific constitutional or hereditary cause. There are other diseases the propagation of which is just as much and just as little traceable as the generation of the living world. If legislation must do something, why should it not direct inquiry in reference to this distinction; and, when it has made out a few facts regarding those diseases that are traceable, why should it not make a trial to lessen or extinguish them? There is a disease of diseases which a mock morality almost makes it a crime to name, but which leaves its base imprint more or less marked, and engrafts a host of evils on a large portion of our people. This of all diseases should be preventible by coercive legislation, if such legislation could do anything; but it cannot. In short, in relation

to sanitary science legislation can do but one thing, namely, promote scientific education. And if it could do this, it could speedily do all that need be done.

Up to this point it has been my object to cast in retrospect something of the past in sanitation. To my mind there is much to be thankful—nay, very grateful—for, and I have no doubt as to the fact that the country is much healthier in mind as well as in body—owing to the efforts of sanitary reformers—now than it was half a century ago. It will be seen, however, that I do not attribute this so much to legislation as to education, and that the prospect in advance lies in the direction of enlightenment of the masses rather than in any attempts to force them by vehement measures into perfection of life. Compulsion appears to me to be out of date. There are thousands of people who do not wish to live, and would not thank us for saving their lives; there are tens of thousands who oppose every sanitary measure because it is an innovation, and is contrary to what they have been brought up to; there are hundreds of thousands who never give sanitation any assistance without feeling that it involves some unnecessary infringement of their rights or comforts; and there are millions who never think at all on the subject. To all these legislation is an enforced penalty to be resisted; they all require to be taught, not ruled. To them the Government is like a schoolmaster who goes about with nothing but the cane.

THE WORK OF THE FUTURE.

Our task as sanitary reformers for the future lies in the art diffusing knowledge into every nature, however dense, capricious, obstinate, sullen, or indifferent; and for this task the office of sanitary inspector is par-

ticularly valuable, apart from its direct intention. I would open to inspectors a new sphere on a large scale ; I would like to see them playing the part of elementary teachers of truths relating to health. They need not burden themselves with the work of original investigation ; they may leave that to the learned pursuit of the medical officers of health. Their duties as the teachers of a new branch of science may be confined to the communication, to others less favoured, of the information which they have acquired. They are a strong body distributed throughout the length and breadth of the land ; they have unparalleled advantages of reaching persons who have never before heard of sanitation, and that, too, under circumstances when the mind is most impressionable ; and they would acquire in this way an influence which has not yet been accorded to them.

One thing, in addition, they must strive for : they must make known their useful labours for mankind until they have achieved an independency that shall leave them to do their best without danger to their interests or discomfort to their minds and homes. Considering the services they render, considering the dangers to which they willingly expose themselves, they are amongst the worst-paid members of the community ; and the unsatisfactory tenure by which the majority of them hold their office is amongst the most lamentable of public procedures. Happily, the difficulties under which they labour cannot be long sustained when important public bodies and cities are glad to vie with each other in showing how fully they appreciate them and their work.

OPUSCULA PRACTICA.

"There are mites in science as well as in charity."

BENJAMIN RUSH.

ON THE CARE OF THE PREGNANT WOMAN.

FOR many years past I have taught with all the energy at my command the lesson that the pregnant woman is one who should ever be under exceptional care. For the first time I have found this view supported by one of the leaders of public opinion outside the profession of medicine—Mr. W. T. Stead—in a remarkable article by him in the last *Review of Reviews* on, what he calls, the "Civic Church." This is not the place for me to comment on the drift of the article altogether, much as I am bound to admire it, and hopeful as it seems to be of a new and grand reformation; but on the particular subject of the care of the expectant mother I may dwell, because it is a medical as much as a moral subject, and is one on which, independently of its ethical nature, I have ventured to write, approaching as I often do, quite unintentionally, an ethical topic on the purely physical side.

In my first years of professional life, although I gained the Fothergillian gold medal for an essay on the diseases of the child in the womb—"Diseases of the Foetus in Utero"—I did not attach the importance due to the question of the effect of pregnancy either

on the mother or the offspring. In this I was not peculiar. I merely thought what the vast majority of grown-up people think, namely, that the pregnant condition is so natural, it must be borne without any special consideration, and that the woman must go through with it like the rest of her sex, doing her best to bear all she is tried with, thinking of the whole affair with as little anxiety as possible, ignoring the fact that she is, for the time, the centre of a new world, the temple of a new humanity, and continuing to work, eat, drink, sleep, and play just as usual, until the final hour when, in the pangs of labour, she has, perforce, to retire from the world that she may cast a new life upon it. Experience at last led me, away from the thoughtlessness so commonly connected with the condition of pregnancy, to see that the change in the natural constitution which tells the woman she is now two existences is no slight incident, no matter almost of joke, but a solemn transformation for mankind.

The morning sickness, the out-of-the-way predilection for particular foods or drinks, the exhaustion from slight exercise, the sinking, the sensitiveness to internal movements, all these signs—summed up in one of the oldest and most significant of terms,—“the quickening,”—are evidences of modifications in the mother which have the most important bearings on her health and life. That she gets through them, and others that follow, in a way we men never know, never can know, is a miracle so common, we do not recognise it except as commonplace and food for casual conversation.

For the moment let me leave out of sight all accidents to the mother, and speak concerning the offspring, the new life that is being developed and formed to be born. There are two stages to that life,

the embryonic and the foetal, and I incline to the view of Dr. Ballantyne that the earlier or embryonic stage, the developing into form, is probably more important than the later or developed state, when organs are present, and functions are in play. In the embryonic or developing stage, about which so few have the slightest concern, the type and, we may say, character of the offspring is set forth on its journey of life. The life is now wholly dependent on the mother. What she eats and drinks, what exercise she takes, what sleep she secures, what clothes she wears, what surrounding states or conditions affect her mental sphere, all tell on that newly forming entity her coming child; the build, the quality, nay the sex, may be determined here; it is the genesis of the new existence. Perfect build or deformity is at stake, a man, a woman, or a monster of either sex. Now is the time when mental serenities or mental perturbations of fear, anger, jealousy, tell through the mother on what she is bearing for the future.

When the child is formed, when, in its enclosed sphere, it is a human being, has body, limbs, heart and circulation, brain and nerves, glands and secretions, it is under two conditions: it makes its own tissues out of the blood of the mother, and it has its own independency of organic function. How far that independency extends we are ignorant as yet. It is scarcely probable that the direct nervous impressions from the brain of the mother pass now to the child like the blood she supplies, for if they did there would be more frequent death in the womb from shock and nervous exhaustion; but there will be good or bad states in the nervous organs according to the supply and constitution of the maternal blood out of which those organs are formed.

Added to all there are the chances of injury to which the half-independent body is subjected from without. It has had its genesis and it has had its exodus from its primitive mould into organic bodily form. *It is.* It is one human being supported by another and influenced already by every-day environments. It cannot see, but it probably can hear, and when its mother's heart palpitates or faints, its own must share in the varying rhythm. Its voluntary and semi-voluntary muscles may not feel fatigue, but its involuntary ones are as subject to fatigue and rest as if it were born. It has its diseases, affecting largely its skeleton and its outer covering or skin: it can be convulsed; it can, as I and others have seen, be affected by the acute contagious diseases, like small-pox and typhus; it can be an impregnated sufferer from syphilis. I have little doubt it is affected by external vibrations, and it is not too much to believe that on its *tabula rasa* it may receive sounds and even words and descriptions which are treasured there to be recalled in many later days after birth.

We medical men, leaders and teachers of a new era, should bear all these natural lessons in mind and disseminate them, as we alone can, to those to whom we minister. Our knowledge is sacred; and our wisdom, fed by it, should lean to no effort more divine than the protection, by our experience and guidance, of the pregnant mothers and the unborn units of our species.

CARDIAC MURMUR IN THE PYREXIA OF CHILDREN.

OME eight times in my life I have been called into consultation in the cases of children who during a febrile condition from inflammatory or epidemic disease have developed a cardiac

murmur. The cases are fairly typical, and were well illustrated in a patient whom I saw a few months ago suffering from the acute stage of measles. In him there occurred, while the fever was 101.5° F., a loud, sharp, but musical systolic murmur. The practitioner in attendance was much alarmed and anxious about this symptom, thinking it indicated endocarditis and yet being unable to detect any reason for such a complication in what was so apparently an ordinary form of the exanthematous malady. I quieted his fears from my experience that there was no true cardiac affection, and that what we heard was no more than abnormal sound due to increased tension of the bicuspid and tricuspid valves under the influences of high temperature and quickened cardiac action. I predicted, from experience, that, as the febrile condition was reduced, the murmur would pass away, and that there was no danger at hand. The predictions proved correct; and it seems to me a good fact to bear in mind during febrile states in children that the murmur is a common phenomenon, and need not be seriously feared. When first I heard it I was myself alarmed, but since I have traced out its origin and seen its harmlessness all fear has abated when no other sign of cardiac disease has been present. For many years in every instance of pyrexia in children I have examined for the murmur, and have very rarely indeed found it absent. I have also examined healthy children after they have quickened their circulation greatly by moving, and have never failed to get the same murmur for a brief period. The result is exactly what we might foresee. The valves in the heart of the young subject are very elastic and very resonant, while the chest-wall is like a sounding-board; given therefore overtension of the valves, and a musical

murmur is inevitable. To some extent the same is true of adolescents.

When there is real mischief in the heart in children there are other symptoms of moment which indicate actual danger. There is rheumatic symptom, or there is pain in the cardiac region, or there is irregularity of cardiac movement, or there is some difficulty of respiration. These symptoms mark out a definite local condition, and must be accepted as truly abnormal in their nature. In the present communication nothing more is to be included than temporary increased valvular tension dependent on febrile elevation of temperature and the consequent increase of cardiac function.

HOSPITAL STATISTICS IN MADAGASCAR.

DR. S. BACKWELL FENN, of the Medical Mission Hospital, Antananarivo, Madagascar, has sent me some most interesting statistics of hospital practice there. Into the hospital 780 in-patients were received in the twelve months of 1891; and of these 404 were discharged cured, 195 relieved, 35 unrelieved, and 48 dismissed for other causes, such as refusing control. There were 45 deaths in the institution, yielding a death-rate of 5·87. This death-rate includes all cases dying in the hospital; but if the cases dying within two days of admittance had been excluded, the death-rate would have been 2·97. The above classification does not include cases in the hospital on January 1st, 1892.

The out-patients in 1891 numbered 4,366, and the annual cost for all was about £1,500, or four times less than would have been incurred in an English hospital

of the same size. There are two English medical men and one native assistant attached to the hospital, and there is a medical school, in which there are thirty-five students. All sorts and conditions of patients are admitted.

Dr. Fenn compares these figures with those that have been published by the London Temperance Hospital, the conditions being much the same in that the alcoholic treatment of disease has been abandoned, and alcohol is prescribed in an exceptional and scientific manner as if it were a drug or medicine. He considers that the results are very satisfactory, and makes inquiries as to the mode in which glycerine tinctures are made for hospital use, with the view of introducing them into his practice.

STATISTICS OF DEATH IN HOSPITALS.

THE statistics sent by Dr. Fenn tempt me to refer to the facts published in ASCLEPIAD xxxvi. on two hundred cases treated in the Temperance Hospital without the administration of alcohol. The report on these cases has led to much comment by medical cotemporaries, and very different conclusions have been drawn from them. One critic says that the personal equation must be taken into account in the study of the facts, as if all practitioners had not the same facilities as myself for treatment without alcohol. It may be flattering to be spoken of in this manner, but I must not be led away by it. I have no special facility for practice without alcohol, none that every qualified practitioner may not be possessed of, so that, if my results have been good, they have been so on the ground of entire abstinence from the

use of alcoholic doses. A second critic says that the report of the cases is not sufficiently explicit; that the treatment may owe success to the slight character of the diseases, or to the fact that all the patients were total abstainers. As to these suspicions, I answer that there was no special quality of slightness in the cases. There were some that were very severe, others that were by comparison slight, just as there would be in any other general hospital; and in regard to abstaining patients and non-abstaining there was no rule of exclusion: both were admitted according to date of application and in much the same proportion as abstainers and non-abstainers exist in a mixed community. Here again, therefore, any success must be fairly and honestly attributed to the non-alcoholic method.

There have been other critics who have denied that the treatment has been successful. One, indeed, says that he considers the mortality of 14 per cent. in acute medical cases unusually high, and this criticism concerns me most. I wish to deal with it with the utmost candour. Let me repeat, then, that in my returns I included every case that came under my charge, not omitting those of poisoning or violent death. I also included cases admitted so advanced in disease that they died before any systematic treatment could be brought to their aid. If these cases had been left out of the calculation, the mortality would have been a little over 11 per cent. Now the question is whether 11 per cent of mortality in hospital medical cases is, by comparison with mortality in other hospitals from similar diseases, a high or a low mortality. With submission, and with every wish to stand corrected if I am wrong, I think it is a lower mortality than I have ever seen,

and I hope it is lower than that of any hospital where alcoholics are employed.

One point more. In my desire to be correct, I have re-perused the reports of the post-mortem records in those fatal cases where the post-mortem was permitted, and I can see from them no reason to suppose that the administration of alcohol to the patients could have done any possible service towards saving their lives in their last illness. My conscience, therefore, is clear, and I continue on my way as ever, and shall do so until some more fortunate brother-physician shows me that through alcohol as an ally he can pass me in the race of saving life from imminent risk of death.

A final criticism on these statistics is that if the results we claim from them be ever so good, it is folly to exclude what might be a profitable agent in the way of cure when it is judiciously used, and to exclude alcohol merely because it is alcohol, because it sometimes does evil when injudiciously used, is a system for which there is no valid excuse. The answer to that objection is that the treatment of disease without alcohol is a justifiable attempt to solve a question about which there is the most obstinate controversy. The thing alcohol has been placed in a false position. It has been considered as a necessary food, not as a medicine or drug. Its value, therapeutically, has been appraised as a food. To cut it out of treatment is, consequently, held to be starving the sick and depriving them of something essential to existence ; and, so strong is this belief that even the prescription of alcohol in medicinal doses would be claimed by its supporters as an admission of its power, and as a proof direct that we who doubt it dare not leave it out of our book. That there may be no mistake, we have no alternative therefore

except to ignore it fully, absolutely. By this means we obtain crucial and unmistakable evidence. If I have erred in testing proofs, without a possible chance of escape from error, then my results, by the side of others in which alcohol has played its part, should be so indifferent as to make the mistake obvious to every one. If, on the contrary, my results are as good—not to be too extreme—as those set against them under alcoholic stimulation, then I have shown that the notion of necessity for alcohol in the treatment of disease is a grand delusion; that at least we can treat disease as competently without it as with it; and that if, at this date, it were being introduced as a new remedy, we could, as we probably should, soon discard it as more trouble than its worth and as another of the thousand new follies, bound to live its short day and become forgotten.

HARD WATER IN WATERING-PLACES.

IN many fashionable and, in many respects, beautiful and healthful watering-places, much inconvenience is experienced from the hardness of the water. I could point to two famous seaside towns where real injury to health is the result of the hardness of the water. The visitor arriving there for change and rest finds himself for a few days much improved, owing to the change of air and repose from work. Then he becomes low-spirited, is dyspeptic, feels himself distended with gases in the stomach and intestines, sleeps indifferently, is constipated, passes a large quantity of pale urine, is somewhat hysterical, and declares that the place, much as he may admire it, does not suit. Sometimes he feels

pain in the stomach, rising up on the left side to the shoulder, and speaks of his liver being sluggish. He takes alteratives, but they do not relieve him, and he leaves, condemning the place for some fault he does not understand. In nine cases out of ten, all these symptoms are due to hardness of the water, and to nothing else.

The favourite sea resorts where this fault occurs ought to know this fact. Brighton ought to know it; Eastbourne ought to know it; Bournemouth ought to know it. These towns spend large sums on means admirably intended to make their visitors comfortable; but they fail in this one particular, vital as it is. They fail also without any reason, for they have the means at their command for remedying the evil at once if they would only take the lesson. The governing body of every town where the water is hard should send a committee to the city of Canterbury to learn the simple method of softening the water by the Porter-Clark process on a large scale. Canterbury is not a fashionable watering-place, but it has set the example of rendering healthful one of the grand necessities of life: drinking water. The inhabitants of that city have all their water supplied to them at four degrees of hardness, and the result is excellent in every way.

Hard water produces the temporary symptoms named above. It does more: taken for a long time, it causes derangement in the function of the kidney, and is very favourable to the deposit of stone in the bladder. It also interferes with the proper infusion of vegetable substances, like tea and coffee, and causes encrustation of vessels in which water is boiled. For ablution and for baths, it is very objectionable, and it gives

to the laundress much unnecessary trouble. So much the more important is it, therefore, that in all places, but in resorts of health particularly, it should be properly softened and made a surer means of cleanliness as well as of constitutional invigoration.

HOW TO SOFTEN WATER AT HOME.

THE above opusculum suggests to me to describe the late Mr. Alderman Hallett's mode of softening water on a small scale. It is very practical. Place near together two two-gallon stoneware casks. Fill one with the hard water, a half-pint of limewater being first put in. After standing twenty-four hours the supernatant water will be as clear as at first, and at the bottom of the vessel will be found a precipitate of chalk. The shape of the vessel is best if cylindrical, with the tap-hole a short distance up the side. This form of vessel allows the process to be completed within twelve hours. The second cask (or vessel) is provided to ensure a reserve of softened water while the other is being treated. No weighing of lime is required. The limewater is obtained by putting into a stoppered bottle fresh caustic lime, and water is to be poured on it to fill the vessel. In a few hours the upper part of the fluid is quite bright, and is saturated with lime. The charge of caustic lime need not be renewed oftener than every two or three months. If it is objected that the quantity is small, more vessels can be used, or larger ones; so as to meet the requirements. There is also an advantage in having the water only stored a day or two during hot weather, since on standing it soon ceases to have a brisk taste. Persons who

are liable to irritation of the mucous membrane from hard water, quickly derive benefit from the continued use of this softened water for drinking purposes.

The softening process might easily be adopted by laundresses by using large casks; the saving in soap would well repay them for a little trouble.

1 cwt. lime will do the work of $20\frac{1}{4}$ cwts. soap.

Cost of 1 cwt. quicklime, 8d.

Cost of $20\frac{1}{4}$ cwts. soap, £47 1s. 8d.

There is, therefore, very little question that the adoption of some such mechanical means of mixing, combined with a rapid filtering of the separated chalk, is soon paid for through the large saving in soap alone.



THE OVERCHARGED BRAIN.

REMEMBER watching a young man who had been preparing for graduation in honours for one of our universities; I remember watching a young lady who had been competing for a first place in the high-class university examinations. I looked at the lessons these pupils had been forced to learn, at the charges of knowledge they had been obliged, in a given time of short duration, to stow away in their brain, which charges must, when the examiner pulled the trigger, be exploded on the examination paper; I looked at one particular subject of examination, with which I was best acquainted—for it included some researches of my own—and I asked myself, “How long would it take me, who am a fair hand at learning anything I may apply myself to, to get up sufficient information to answer all the questions that are set for these young minds to answer?” The conclusion I

was forced to arrive at was that the task on the one subject on which I was best able to form a judgment, because it has been a pursuit of my life, would be more than sufficient to fill properly all the time that was given to learn the whole examination programme. I saw, in short, a trained specialist in one department of learning putting forth his strength to wrestle, on his own ground and on his own conditions, with a young brain that had been straining to get up a similar contest with a number of other trained specialists, not one of whom could wrestle with his colleagues off his own ground. The process amounted to an inquisition, and it were better for the upper and middle classes if there were no competitions at all than that such a pressure should be inflicted on their children. To have sent a Faraday, a George Stephenson, a Fergusson, a John Hunter, a Newton, a Shakespeare, a Bacon, through any such ordeal, would have been either to have whipped them entirely off the stage of the literary world or to have made them the merest pedants. In time, as Nature declares herself with her clear voice, the blunder will stand out too distinctly to be unrecognised. When it is seen, as it will be seen in another generation of manhood, that humble board schools, teaching the "three R's," are the best training schools for the genius and mental greatness of the country, and that the men and women who have been pressed into the university service are beaten into fits by their lower and freer neighbours—are, in fact, nurtured imbeciles compared with their lower and freer neighbours—then the follies of many existing plans will be discovered, and the rectification of them will be the earnest question of the day. At present it would seem that matters must stand as they are. The university scholars have it all their own way; they have no competitors as yet in the race, and no comparisons can be

made of a practical kind until the competitors from the lower schools are on the field. We can therefore only look in advance to see what will happen, and predict the future from the psychological and physiological lessons which lie before us for our guidance. These, fortunately, are decisive enough, and they are helped by the facts which the history of man has given for our edification. The lessons show us that young brain unduly exercised becomes old altogether while it is yet young, and that old brain moderately exercised remains young in parts when as a whole it is old; they show us that the mind which will throw off the most brilliant work, the mind that will master the most difficult inquiry, the mind that will lend itself most readily and earnestly to the hardest tasks, is the mind that has simply been put in the way of doing work, and which, possessing the elements of learning—reading, writing, calculating—has been left to its own freedom.

THE DREAD OF "PLUCKING."

HE modern student of the middle and upper classes of society during the time he or she is preparing for examinations, which means the most critical time of his or her life in respect to health, finds no time whatever for healthy recreative pleasures. There is no possibility of such a thing. The exigencies are so pressing, the dread that failure on a single subject may cause "plucking" is so incessant, the mind has really no peace except when the eyes are bending over the book and are straining to take in still more and more of the hard lesson. The anxiety brings sleeplessness; the sleeplessness increases the anxiety; then the body becomes feverish; and at last, when the great effort has been made, the result, whichever

way it turns, is inevitably bad. To win is to secure a passing advantage bought at the cost of some years of active life, and with the mental faculties forced into grooves out of which they never quite free themselves. To lose is to be subjected to a chagrin and annoyance which in sensitive natures runs actually into remorse, shame, or a dazed imbecile carelessness which, in a person past maturity, would lead to dementia, and which, in the young, is at no time fairly recovered from and never fully forgotten.

I am perfectly alive to the fact that, in writing what I have written above, I am stating some very hard and unpleasant truths; but "if an offence come out of truth, better is it the offence come than the truth be concealed." I am but telling as a true physician what experience teaches me of the physical conditions induced by the excessive and useless exercises of labour to which the younger and as yet better brains of our community are subjected; I am but telling the experience of other members of my profession who feel as I do, if they do not express themselves so freely. After a lecture I once delivered in one of our large provincial towns, and in which I spoke what I have here committed to paper, one of the eminent physicians of the place came to me afterwards to thank me for the statement and to give me the following personal experience. "My own son," he said, "a youth of good intellectual parts, made up his mind to go in for his degree at a university"—naming the university in which, in this kingdom, the process of intellectual destruction is most scientifically and systematically carried out—"and determined also, if it were possible, to win a good place in the lists of honour. He worked through all the preliminary grades with untiring industry, taking no rest and unable to find time for any.

He went in for his great effort, lost, and now is wandering on the Continent utterly broken down mentally and physically, more like an idiot, for the time, than a youth of good intellectual capacity originally, on whom every expense has been bestowed to ensure for him a good education."

These results of their system they who plan and carry out the examinations do not see. They are intent only on the sustainment of what they honestly consider to be the credit of the university, the maintenance of its high character, so that none but brightest scholars should appear on the rolls of its graduates. The examiners look purely, as their duty, for the direct efficiency of those who come before them for examination. Neither manager nor examiner can ask after the antecedents of those whom they take in charge. They do not know that the student may be the subject of tubercular diathesis, of heart disease, or brain affection; and that out of every dozen students one at least will be under some such disqualifying hereditary influence. They proceed, adding difficulties upon difficulties, as if every student had the same stamina and the same capacity.



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Ro. Boyle

*Autotype from an engraving
by R. Woodman after a
painting by Kerseboom.*

Boyle

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THE HONOURABLE ROBERT BOYLE, F.R.S.

“ R. BOYLE, the ornament of his age and country, succeeded to the genius and inquiries of the great Chancellor Verulam. Which of all Mr. Boyle's writings shall I recommend? All of them. To him we owe the secrets of fire, air, water, animals, vegetables, fossils, so that from his works may be deduced the whole system of natural knowledge.”

This tribute to the genius of Robert Boyle, who was born the same year as the great Chancellor Verulam died, was paid to him by Boerhaave, and served as a kind of supplement to what had been said of him in a funeral discourse by the celebrated Gilbert Burnet, Bishop of Salisbury, who, speaking of Boyle as a philosopher, declared, “His design was only to find out nature, to see into what principles things might be resolved, of what they were compounded, and to prepare good medicaments for the bodies of men. He spent neither time nor fortune upon the pursuits of high promises and pretensions; he always kept himself within the compass that his estate might well bear; and as he made chemistry much the better for his dealing with it, so he never made himself the worse or the poorer for it. It was a charity to others as

well as an entertainment to himself; for the produce of it was distributed by his sister and others, into whose hands he put it."

In plain words, Boyle was one of the most fortunate of the fortunate men of science. Labours in scientific research were not wanted to give him station, for he was born to a station which no mere man of science could fully acquire, whatever he might do to deserve it. He had means at command sufficient for all his experimental necessities; and he had the privilege of descending with the advantages both of science and of nobility for a birthright, the happiest of combinations that could fall to a philosopher inquiring into natural things. "*Felix qui potuit rerum cognoscere causas,*" never was better exemplified than in his life and career.

The father of Boyle, Richard Boyle, favoured by fortune and still more by his own efforts, became Earl of Cork, and in time was still better known as the "great Earl of Cork," whom the King, Charles I., credited with having, as governor of the province of Munster, in Ireland, shown himself "to be a person of great abilities in building towns, and fortifying them with fair walls and towers, and filling them with English colonies, building churches, and reducing the people to civil obedience; in establishing religion, extirpating superstition, defending the passes of the country with castles, building many bridges for the convenience of the public, guarding the ports and maritime places of the province against foreign enemies; in first introducing manufactures and mechanic arts into the province and afterwards establishing them by guilds and fraternities of artificers, to the plentiful increase of riches and civility; and all this at his own expense and by his own industry."

The wife of this earl was Catherine, daughter of Sir George Fenton, principal secretary of state, and a mother of such excellent nature that her husband, who was knighted on their wedding day, said of her, "I never demanded any marriage portion with her, neither promise of any, it not being in my considerations; yet her father after my marriage gave me one thousand pounds in gold with her. But that gift of his daughter to me I must ever thankfully acknowledge as the crown of all my blessings; for she was a most religious, virtuous, loving, and obedient wife to me all the days of her life, and the mother of all my hopeful children."

Such was the happy and immediate pedigree of Robert Boyle, who, the seventh son and fourteenth child of this union, was born at Lismore, in the province of Munster, on January 25th, 1626-7. For natural nursing he was put under the care of a country-woman, who was instructed to treat him as if he were her own child, his father having, as he said, "a perfect aversion for the fondness of those parents who breed their children so nice and tenderly that a hot sun or a good shower of rain as much endangers them as if they were made of butter or of sugar." It does not, however, appear that Robert Boyle was favoured by the parental resolution. He was by nature of a gentle and, we may almost say, fragile constitution, and was rather unfortunate in his first years. He lost his mother when he was three years old; he mocked some children or playfellows who stuttered, and so completely caught their peculiar habit that he was never cured of it, though every endeavour was made in that direction; lastly, while a child he was subjected to an extreme nervous shock by being thrown in a coach into a flowing stream, to be rescued by one of his father's gentlemen.

In his father's house at Lismore Boyle received his first lessons, the chaplain of the house teaching him writing and arithmetic and a French gentleman resident there instructing him in languages. This tuition continued until his tenth year, when he was sent to England, in order, under the direction of Sir Henry Wotton, to be educated at Eton. Here he imbibed, according to his statement, a taste for other than pedantic books from accidentally reading the story of Quintus Curtius, to whom he owed more than Alexander did, and derived greater advantages from the history of that great monarch's conquests than ever the monarch did from the conquests themselves.

At Eton, again, he was subject to one or two physical accidents, but remained there nearly four years, and was then taken by his father to their seat at Stalbridge, where he was placed under the care of a clergyman of Stalbridge, who acted as chaplain to the family. In 1638 he moved with his father to London, residing at the Savoy and continuing his studies until his brother Francis married Mrs. Elizabeth Killigrew, after which event he and Francis started off on the grand tour, according to the fashion of the day, accompanied by a guide and tutor, Mr. Marcombes. At that time the old town of Rye, in Sussex, was a place of embarkation for packets sailing for France, and from Rye they passed over to Dieppe, thence to Rouen, Paris, Lyons, and Geneva, stopping at the last-named place in the house of their tutor, for student work. Boyle while here pursued the study of mathematics, which he had commenced at Eton in a kind of retirement from the perusal of romantic books supplied to him while recovering from ague. He did not, however, devote

all his time to study; he rambled about a great deal, travelled in Savoy, and reached Grenoble, in the Dauphine province. He seems at this period of his young life to have been bitten of melancholy, "the devil taking," he tells us, "advantage of it"; and the strange stories and pictures he formed of Bruno, the founder of the Carthusian monks, "suggested such curious, hideous, and distracting doubts of some of the fundamentals of Christianity that, though his looks did little betray his thoughts, nothing but the forbidness of self-despatch hindered his acting it." The melancholy, bordering evidently on suicide, acted upon him for many months, and led him, after he got out of it, into an inquiry as to the foundations of the Christian religion. By this means he became confirmed in his belief, "although," to use his own words, "the fleeting clouds of doubt and disbelief did never after cease now and then to darken the serenity of his quiet, which made him often say that injections of this nature were such a disease to his faith as the toothache is to the body, for, though it be not mortal, it is very troublesome."

Quitting Geneva, Boyle proceeded to Italy, and wintered in Florence, where he picked up the Italian tongue and worked very sedulously at Italian literature, especially that of a scientific kind. He was much influenced by the writings of Galileo, and it was while in Florence that Galileo himself, by this time deaf, hypochondriacal, and rheumatismal, terminated his eventful and sad yet beautiful life in a residence near to the city. It does not seem, however, that the young and the old philosopher ever met. From Florence, after a rather long sojourn, he proceeded to Rome, having sighted the Carnival, which he looked upon as a species of lunacy, saw the Pope (Urban VIII.) at

chapel, and made many explorations into places surrounding the Holy City. Remaining in Italy until 1642, he began to make his way Englandwards, receiving on his journey mournful news from his father respecting the rebellion in Ireland and finding, when he reached his native shores in 1644, that his father was dead, and he himself the owner of Stalbridge, of estates in Ireland, and of considerable wealth in money. From henceforth he was independent; and, great as was his enthusiasm for research and free as he was with money for that noble end, he never exceeded his income, and never in any excess or frivolity set aside the claims of the sciences he most loved. Even his chaste love for one fair lady he conquered, and to the end of his days remained a bachelor.

On his settlement in England he first retired to Stalbridge, and all through the troublous times of the Civil War, in which he took no active part, he remained a student, now in London in the house of his sister, the Viscountess Ranelagh, in Pall Mall, then in Oxford, and for a short time in Holland. During this period, so memorable in the history of our country, he became a central figure in a society which met in London or Oxford under the name of the Philosophical College. The members of the college held their meetings in the quietest, if not most secret, manner, and he aided the learned body to hold together until after the Restoration, when they became the famous Royal Society, the home ever since of all that is most useful and most famous in English science.

A very young man when he came into his patrimony, Boyle quickly sobered down into a calm, philosophic frame of mind; so that, in truth, it seems as if he never had a youthful time or prime. He was the friend, and

companion, and fellow-labourer of men who were his seniors from the first. He followed no profession; and although the Church for a time tempted him, he felt he could not, in conscience, give himself to the vocation; yet he wrote works which would have distinguished a professed cleric as a theologian. He did not become a chemist in the strict sense of the term; yet he made many chemical discoveries, or helped to make them. He did not court medicine as a professor or practitioner; yet he was so fond of medical art and, indeed, of medical research, he fairly comes under the title of medical philosopher.

MEDICAL WORKS OF BOYLE.

In the space at my command I must refer only to those labours of our philosopher which have a bearing more or less direct on medical subjects. A catalogue of his complete works would fill half a number. Birch's "*Life and Collectanea*" of Boyle includes five ponderous volumes in double columns, each volume extending to nearly six hundred pages. Out of these we are specially concerned with the following essays: (a) Experiments physico-mechanical touching the *spring of the air* and its effects and touching the *weight of the air*; (b) Certain physiological essays; (c) The usefulness of *natural philosophy to physic*; (d) Particulars relating to the *pathological part of physic*; (e) Particulars relating to the *semeiotical part of physic*; (f) some things relating to the *hygienical part of physic*; (g) Particulars wherein *natural philosophy* may be useful to the *therapeutical part of physic*; (h) New experiments and observations touching *cold*; (i) New *thermometrical experiments and thoughts*; (j) "*Observables*" upon a *monstrous head*; some anatomical obser-

vations of *milk* found in *veins* instead of *blood* and of *grass* found in the *windpipes* of certain animals; (k) The method observed of *transfusing* the *blood* of one animal into another; (l) *Trials* proposed to Dr. Lower to be made by him for the improvement of *transfusing blood out of one live animal into another*; (m) A confirmation of the experiments made by Signor *Fracassati* in Italy by *injecting acid liquors* into the *blood*; (n) New experiments concerning the relation between *light* and *air* in shining *wood* and *fish*; (o) New *pneumatical* experiments about *respiration*; (p) New experiment about the relation between *air* and the *flamma vitalis* of animals; (q) An attempt to produce *living creatures in vacuo Boyleano*; (r) An account of the two sorts of the *Helmontium laudanum*; (s) *Memoirs* for the *natural history* of the *human blood*, especially the *spirit* of that liquor; (t) Observed causes of the *insalubrity* and *salubrity* of the *air* and their effects; (u) *Medicina hydrostatica*, or hydrostatics applied to the *materia medica*; (v) Duty of a *mother* to *nurse* her *own child*; (w) *Chemistry*: its usefulness to *human life*, to *trades*, etc., and *chemico-medical chaos*; (x) Of *poisons* and of turning them into *antidotes*; (y) A "practical essay about *vitiated sight*" and an invitation to physicians to make use of simple medicines; (z) A chemical and mechanical examen of *materia medica*.

We discover in this remarkable alphabet a little library of medical philosophy. In some instances matters in their origin are unfolded; in other instances the subject is glanced at rather than expounded; and, in a third set, nothing is really effected from which any value has been derived up to the present time. Out of the alphabet that has been presented,

however, there is much to learn, and to some of the heads of it I proceed briefly to invite attention.

SPRING AND WEIGHT OF THE AIR.

The essay of Mr. Boyle on the spring and weight of the air was addressed in the form of a letter to "Lord Dungarvan, his honoured and dear nephew," then (1659) residing in Paris. The communication, extending really into a volume, is the narrative of a most careful series of experiments, in which he showed that the air had weight; that by its weight, through an atmosphere which extended above us perhaps fifty miles, it compressed its lower parts, in which we live, thereby condensing itself; that it pressed equally on all bodies in all directions; and that, as in the Torricellian vacuum experiments, it admitted of being renewed or exhausted from glass receivers, according to the method invented by Otto Gericke, in the engine to which the name of "air-pump" was subsequently given, an engine which Dr. Hook aided greatly to simplify. All the experiments read to this day fresh and clear, but those which are most curious to the medical mind relate to respiration which, until they were performed, was a chaotic mystery. In order to see what part the air played in the act of life, Mr. Boyle placed flying insects in a globe that could be exhausted more or less of its contained air. He found that when the air was exhausted the insects fell. He replaced the insects by small birds, then by mice, and in time he found that the animals died from the circumstance that they could not exist unless they had a given quantity of air on which to subsist. *In vacuo* they became convulsed and ceased to breathe. The phenomena induced might, he thought, admit of being accounted for by

the expansion of the blood within the vessels of the animals, as if the blood boiled in them and, as it were, underwent explosion. χ Later he came to a nearer conclusion, namely, that the failure of respiration took place and led to the fatal results because there "is some use of the air, which we do not yet so well understand, that makes it so continually needful to the life of animals. *Paracelsus indeed tells us,*" he continues, "*that as the stomach concocts meat and makes part of it useful to the body, rejecting the other part, so the lungs consume part of the air and proscribe the rest.* So that, according to our hermetic philosopher, it seems we may suppose that there is in the air a little vital quintessence (if I may so call it) which serves to the refreshment and restoration of our vital spirits, for which use the grosser and incomparably greater part of the air being unserviceable, it need not seem strange that an animal stands in need of almost incessantly drawing in fresh air." Until I became familiar with the works of Boyle I was not conversant with the fact that Paracelsus had so ably anticipated the discovery of oxygen. It is clear that from him Boyle had all but seen the secret of oxygen, and what is more curious still, he had knowledge of an experiment of "a famous mechanician and chemist, Cornelius Drebell," who made for King James a vessel to go under water, the trial of which, "with admired success," was conducted in the Thames, the vessel carrying twelve rowers, besides passengers. How this was done he also explains, as he heard from the relatives of the experimentalist. Drebell conceived that it is not the whole body of the air, but a certain quintessence or spirituous part of it, that makes it fit for respiration, which being spent, the remaining

grosser body or carcase (if we may so term it) of the air is unable to cherish the vital fire in the heart. Drebell, therefore, besides the mechanical contrivance of his vessel, had, says Boyle, a chemical liquor which he would from time to time allow to give off vapours that would relieve the troubled respiration of the confined persons, either by restoring to the air some of its vital parts, or by dissipating or precipitating the grosser exhalations. What this liquid was, Drebell held secret, to his own great loss in historical fame and honour.

To Boyle this invention, whether it were a discovery or a trick, seemed to start many and new thoughts which led him to soundest of sound experiments that to this day live. He learned by experiment that the air supported flame, that flame would cease when air was withdrawn; he compared the flame of a candle to the flame of life; and he began to wonder from what source fishes could obtain the vital spirit. He speculated how the *fœtus* could live *in utero*, and he gathered together many facts relating to the length of time famous divers and others could exist under water, recording the instance of a corpulent man who could descend to the bottom of the Thames, and bring out of deep holes at the bottom of the banks "large fishes alive in his hands." Lastly, he drew from his experiments the fact that insects in a reservoir from which air was excluded were rendered temporarily dead, but being exposed to air again, revived, and were restored to all the functions of life.

I would like to follow further many of Mr. Boyle's researches in this essay, and specially those which relate to the spring and weight of the air and to rarefaction; but it is necessary to pass on to the next topic that most concerns us in our vocation as healers.

NATURAL PHILOSOPHY AND PHYSIC.

His essay on the usefulness of natural philosophy to physic is remarkable. He tells us that in his day medicine was divided into five parts: (1) the physiological; (2) the pathological; (3) the semeiotical; (4) the hygienical; and, (5) the therapeutical. We scarcely could divide better in these days, and it is in relation to each of these divisions that he endeavours to show the usefulness of philosophy. He follows Aristotle in saying that the naturalist ends where medicine begins, and medicine begins where the naturalist ends. Under physiology he gives us much information we should scarcely have expected him to have had. He relates instances where the spleen has been extirpated without any apparent injury to life; he tells us that the frog will live and move for a long time, as if not injured, when the heart has been removed from the body; that the viper will move for a long time and appear to live when the head, heart, and intestine have been removed; that the silkworm butterfly is capable of procreation after the loss of its head; and that portions of muscular substance could be digested by the action of acid menstruums (derived from vitriol) in some similar manner to the digestion that occurs in the stomach.

In the pathological part of this discourse he is not so full as in the physiological; yet he recognises the value of pathological research, makes many useful observations on the character and mode of construction of the calculi of the bladder and gall-duct, on fermentations as causes of febrile states, on transfusions into the veins, and on the effects of various poisons absorbed into the body by the veins, the capillaries,

or the digestive canal. The part bearing on semeiological studies is also poor, nor can I find anything in the hygienic section that calls for praise; but of the therapeutical more may be declared, if little in these days can be learned from it. The author's remedies, including urine as one of them, would be a subject for contempt to us moderns; but some facts, as, for instance, the use of galls as a styptic, of arsenic, and of bismuth, call still for our attention. Perhaps the best notes refer to the effects of the mind on the body and of bare motions or active impulses of external bodies in their actions on animal bodies. He tells us what great and dangerous sufferings he has himself endured in sea-sickness, which disease is caused by external impulse or motion entirely, without any fermentative change in the body itself. Motion is here the cause; rest from motion is the cure. Thus a clue is supplied for the treatment of many diseases by rest alone. On the other hand, there are some diseases arising from poisonous influences acting on the body, which are, or seem to be, curable by the aid of external vibrations. The biting of that "poisonous spider" the tarantula leads to a lasting and formidable disease, which, mocking all other remedies, is by nothing so successfully opposed as by music. He speaks likewise with much commendation of the acid fumes of sulphur, administered by inhalation, in cases of consumption of the lungs.

TOUCHING COLD.

A section, very prolonged, on the "Experimental History of Cold" is an almost forgotten work of supremest interest and importance. It is in our time just as curious as it must have been in that of the

author. Especially is it curious to the psychologist and physician. In one part Boyle describes how persons exposed to fatal degrees of cold are sometimes killed, and how the fatal effect is brought about by one of two methods. There are some who die an immediate and painless death. They become slowly benumbed and drowsy, and if they are not roused to exercise, die in torpor or sleep. There are others—and this applies particularly to soldiers who ride on horses and carry armour on their bodies—who are first affected in their kidneys and intestines. They are stricken with pain in these parts, and when they are relieved from exposure they are maddened with the suffering they experience in the abdomen. They are unable to swallow anything without the occurrence of vomiting, and they sink into death with continued griping and cramps, from which there seems to be no relief. After death in these last-named instances the intestines are found to be agglutinated.

The effect of extreme cold in preserving dead animal tissues from decomposition is made much of, and it is certain that if Boyle's observations had been attended to we should not have been obliged to wait two hundred years or more for the introduction of the method of transporting frozen animals from one part of the planet to the other. He knew right well those experiments which show that some living animals, like fish and frogs, entrapped suddenly by cold in freezing water, may be congealed by cold quite through their bodies, and taken out of their imprisonment to all appearances dead, may, under careful thawing, revive and live again. He was inclined to believe that swallows may exhibit the same phenomena ; but was not sure on this point, a reserve which was well worthy of his acute learning.

The swallows congealed by the side of frozen rivers might, he suspected, never have been frozen through, but, merely held in a state of torpor or hybernation, had risen from nothing more than a kind of deep sleep. This would accord with some of my own experiments, in which torpor produced in birds rendered them for the time apparently dead, but proceeded no further than torpor, the beat of the heart being detectable during the whole period. This also would agree with the now well-known fact that, whereas the natural temperature of the fish and the batrachian is lower by several degrees than that of man, the temperature of the bird is several degrees higher, so that between a fish and a bird there may be a difference of as many as thirty degrees on Fahrenheit's scale.

Lastly, Boyle determined some effects of extreme cold on dead animal tissues and fluids. He produced congelation of the eyeballs of dead oxen and sheep, observed that the lenses were made to adhere, noticed that the diaphanous quality of the lenses was lost, and detected that in the process of thawing the water glassy humours seemed to be made of flakes. Further he froze blood drawn from a sheep, and registered in his journal that the blood froze at the top and on all the sides of the dish, and that this ice being separated and thawed at the fire, and again exposed, congealed (coagulated) into a seeming membranous substance, and so continued.

A MONSTROUS HEAD; SPIRIT FOR PRESERVATION.

A short paper entitled "Observables on a Monstrous Head" is of some importance in itself. It tells simply: first, that he saw the head of a colt that had no sign of any nose, unless a double bag that grew out of the

midst of the forehead were some rudiment of it; next, that the two eyes were united into one double eye, which was placed in the middle of the brow, whereby the two eyeholes in the skull were united into one very large hole, into the midst of which from the brain entered one pretty large optic nerve, at the end of which grew the large double eye, the sclerotic membrane which contained both being one and the same, but appearing to have a seam, by which they were joined quite round it, the fore or pellucid part distinctly separated into two corneas by a white seam that divided them, each cornea seeming to have its iris and pupil distinct; also upon opening the cornea there were found two crystalline humours, very well shaped. There were four eyebrows.

To this paper there is attached, as a novelty the remark that Boyle, in order to preserve this specimen, had it put into a vessel and covered with spirit of wine. It is claimed that such mode of preservation was invented by Boyle so that the parts of animals and monsters could be preserved long enough even in summer to afford anatomists the opportunity of examining them.

WHITE BLOOD IN THE VEINS.

A curious person wrote from Paris that on opening the vein of a man in the house of a physician there milk instead of blood flowed. This information being imparted to Mr. Boyle, he replied that the same phenomenon had been observed by a learned physician of his acquaintance. The physician in question was the famous Dr. Richard Lower, to whom I shall have to refer a little later on. The observation of Lower was made on a girl from whom blood was let from the foot.

The first blood on being caught was seen to be very white; and the last, caught separately in a saucer, turned white immediately "like the white of a custard." Five or six hours afterwards Lower chanced to see both bloods. The first was half blood and half chyle swimming upon the blood like serum, white as milk; the second, in the saucer, was all chyle, without the appearance of one drop of blood. When both specimens were heated distinctly over a gentle fire, they hardened, as the white on an egg does when it is heated or "just as the serum of blood does with heating, but far more white." "This maid," it is added, "was then in good health, and was only let blood because she never had her courses, yet was she of a very florid and clear complexion." The blood was let about eleven o'clock in the morning, after a good breakfast partaken of at seven, four hours previously.

These, I believe, are the first clinical observations of chylous blood ever recorded. They were sent to the Royal Society, and were printed in the *Philosophical Transactions*, No. VI., p. 117.

TRANSFUSION.

In the *Philosophical Transactions*, No. XX., p. 353, Boyle publishes a clear account of the early practice of transfusion. He was not, as I have elsewhere shown, the first in this field of discovery, but he was one of the first who reported on the practice of the art of transfusion. Libavius comes first as the expositor; Dr. Richard Lower comes first as the practitioner. From Lower Boyle was inspired; and seeing, as he believed, great advantages from the process, he proposed its further application, suggesting "to exchange the blood of old and young, sick and healthy, hot

and cold, fierce and fearful, tame and wild animals, and that not only of the same, but also of different, kinds." It is worthy of notice that in the practice of transfusion in the lower animals—for he did not make any trials on man or woman—he transfused arterial blood from one animal into the venous system of another animal. He transfused, as a rule, the blood spirting from a carotid artery into the descending part of a jugular vein, leaving the upper part of the vein free, so that the arterial blood, making its way over all the venous system, replaced what was being lost from the open vein. In some instances he seems to have bled three or four animals into one animal, and he adds that the most probable lesson of this experiment may be conjectured to be that one animal may live on the blood of another; and, consequently, those animals that want blood or have corrupt blood may be supplied from others with a sufficient quantity and with such as is good, provided the transfusion be often repeated, by reason of the quicker expense that is made of the blood. In his experiments quills were used as the injecting tubes,—sometimes quills with fine metallic endings,—which were tied into the divided vessels.

The importance attached by Boyle to transfusion is fully conveyed in a series of suggestions which he addressed to Dr. Lower for further experiment. These are sixteen in number, and abound in thoughts of the most curious character. Would transfusion change the disposition of individual animals of the same kind? Would it change the affections? Would the blood of a mastiff frequently transfused into that of a greyhound or a spaniel prejudice the sense of scent? Would acquired habits be destroyed or impaired by the experiment? Would any considerable change occur in the pulse,

urine, and other excrements of the recipient animals after the operation? Whether a well-fed animal yielding its blood to feed a starving animal with its blood would confer loss of appetite; and, whether the blood of the recipient would contain chyle derived from the "emittent"? Whether an animal could be kept alive without eating by the frequent injection of the chyle of another? Whether an animal sick of some disease imputable to the mass of blood may be cured by exchanging it for that of another; and, whether a sound animal may receive such diseases as are not infectious from the blood of a sick one? What would be the effect of frequently stocking an old and feeble animal—a dog—with the blood of young ones, as regards dulness, drowsiness, and *vice versa*? Whether a small young dog, by being often fresh-stocked with the blood of a young dog of a larger kind, will grow bigger than the ordinary size of his own kind? Whether any medicated liquors may be injected together with the blood? Whether, if a purgative medicine be given to the emittent animal, the recipient will be purged, and how? Whether transfusion may be successfully practised from the blood of an animal of one species into that of another species: that of a calf into a dog or of a cold-blooded into a warm-blooded animal? Whether the colour of the hair or feathers of a recipient animal will, by repeated operation, be changed into that of the emittent? Whether by frequently transfusing into the same animal the blood of some animal of a different species something further, and tending to a change of species, may be effected, at least in animals near of kin? Whether transfusion may be practised on pregnant animals, and what effect it will have on their young?

The process of transfusion of blood from one animal

into another soon found its limitation. It fell into an obscurity which it did not deserve, out of which even in this day it has not emerged. Boyle was enthusiastic on the subject, and it would appear that he penetrated in thought some centuries in regard to it. He also was led by it to experiment on the coagulation of blood ; and referring to Signor Fracassati's attempt to inject acid liquors into the blood, he shows that acids like aquafortis, hydrochloric and sulphuric, cause coagulation, while alkalies, especially the volatile alkali, added to warm blood, not only keep it fluid, but suspend its putrefaction. Altogether, in these matters of transfusion and coagulation of blood, Boyle made distinct advances, for which he deserves our grateful remembrance.

PNEUMATICAL EXPERIMENTS ABOUT RESPIRATION.

I must pass over the experiments on the relation between light and air in shining wood and fish, in which, to a large extent, he anticipates the theory of eremacausis, in order to proceed to the more purely medical study, and so I am brought to the work of this master in pneumatical experiments which have reference to respiration. Up to his day it was not fully understood that a constant supply of air inspired and expired was essential for the continuance of life. He proved the fact, by the use of the air-pump, so clearly that the point was never seriously contested afterwards ; he proved it also by his observations on men who were accustomed to dive under water, showing that the best of them were unable to live submerged for more than two to three minutes. He also paid much attention to the vexed question how the air is drawn into and expelled from the lungs. He did not take into account

the elastic tissue of the lungs, but he gave to the diaphragm its proper function; and, contrary to the common view that by some process air is forced into the lungs as it might be into a bladder, he maintained that it is drawn in and forced out as it would be in the working of a bellows. He was, at the same time, observant of two or three other facts which threw new light on respiration. He was aware that in expiration water vapour was systematically exhaled in very considerable quantity, and that in addition there was some other excrementitious substance, as gas or vapour, that commingled with the vapour of water. Thirdly, he noted that when an animal was confined under a jar or in a limited space it lost in bulk, or, as he explained it, in his mechanical way, it lost its "spring."

To Boyle the credit must be given of having been the first to observe that blood and milk, and vascular organs like the liver, contain gaseous or vaporous matter in them which can be extracted by the air-pump. He caused astonishment to some onlookers by saying that the fresh blood of a lamb, that had been held fluid by removal of its fibrine, seemed to boil over the sides of the vessel containing it when placed under the bell of the air-pump, from which the air was being extracted.

He carried also his researches to minute forms of animals such as mites, and saw that they died when they were deprived of air, but that afterwards, being left exposed to air, they revived. He placed a fish in a globe or vessel in the receiver of the pump, and exhausting the air, noticed that many bubbles of air rose from the body of the animal, and that at last it seemed to lose indications of life, from which collapsed state it recovered after it had been put into a basin of pure and fresh

water. Strange to say, in spite of this reading from the book of nature, he did not grasp the fact that fish live by breathing air from water, and respire and expire by the gills, as warm bloods do by the lungs: so often may the acutest minds miss the most obvious truths when only half a problem remains unsealed. He knew that there was air in water, he knew that a fish breathed in water and lived, he knew that an animal existing in air lived by the renewal of air, and he knew that the economy of the two sets of animals was virtually the same; yet he missed the connection between the air of water and the lungs or gills of fish. The oversight seems incredible, yet is it not more incredible, I warrant, than a thousand equally plain natural processes we are observing daily now and do not realise the meaning of, however simple they may be.

“FLAMMA VITALIS.”

One of the most original chapters of an experimental character in the works of Boyle is that which he dignifies as the *flamma vitalis*. He was impressed with the physical not less than the poetical idea that life was a flame, and he was led to the physical experiment of testing how far the idea accorded with the fact. He placed small animals, birds, and mice in closed jars, in which a spirit lamp also burned, and he found what we now know happens: that the burning lamp begins to fade and die long before the lamp of life goes out. He tried the same experiment in a receiver from which the air could be extracted by the air-pump; and again he discovered that the *flamma vitalis* outlived the flame of the spirit lamp and of a candle and taper that were substituted for the lamp. He ignited a bit of charcoal, and let it smoulder in

the receiver in company with a small bird, and once more noted that the living fire went out first when air was withdrawn, and that the bird became sick, but on being readmitted to fresh air made a very good recovery. Over these phenomena he was much puzzled, not knowing, with full knowledge, of the deadly gas which is emitted from burning charcoal; but in the end offering a most shrewd guess at the truth that "common flame, making a great waste of the aerial substance, cannot so easily as the other find matter to prey upon, and so expires while there yet remains enough to keep alive the more temperate vital flame."

Touching the vital fire and light, Boyle carried out a series of experiments on glow-worms, and on their light-giving material, by putting them or portions of them into the receiver of the air-pump and exhausting some air from them. When the air was removed he saw the light disappear, and on letting in air again saw it reappear more refulgent than before. He tried the experiment, too, with a dead and a living glow-worm under the same receiver, and found the same result: loss of flame when the air was withdrawn and return of the luminosity when the air was re-supplied. But the luminous part of the dead worm was much larger than that of the living, and the light of the dead one appeared of a greenish blue, whereas that of the living appeared of a whitish yellow.

Another point of deep interest was settled by Boyle. It had been conceived up to his time that an animal weighed more immediately after than immediately before its death. He tested the fact on a mouse and on a kitten, and learned that the reverse was the fact. Both animals after death showed loss of weight, the kitten, which in life weighed between

ten and eleven ounces, having lost by death four grains.

Finally, under this head Boyle tried if silkworm eggs under proper warmth would hatch *in vacuo*, or whether, in the process of hatching, air were required. The experiment was not so carefully watched as others were, and some apologies were made for that circumstance; the conclusion, however, was arrived at that air was again a necessity, and that the vital flame could not be set going in its absence.

I would I could find space for parts of some essays "on effluvioms; on some hidden qualities of air; on the corpuscular or mechanical philosophy; on attraction by suction; on potential coldness; on the mechanical production of odours; on the hypothesis of alkali and acidum; on the mechanical origin or production of electricity; and, on the absorption of air by metals, with increase of weight." In each of these essays we should find food even for modern science, but they must be allowed to pass, so that in the few remaining pages at command those things that are most to our purpose may pass before our minds.

THE HUMAN BLOOD.

I skip over the receipt for making what was called *Helmontium laudanum*, a medicine invented by the son of Baron von Helmont—and which was, in fact, nothing more than opium digested with quince and saffron—in order to treat on the great essay on the "Natural History of Human Blood." In the investigation on which the essay is founded we are led to the study of the following subjects: the colour; the taste; the odour; the heat; the inflammability; the aerial particles; the specific gravity of the red and

fibrous parts of the blood, and of the serous part; the consistency; the disposition to concretion and the time wherein it is performed; the liquors and salts that coagulate and those that impede or dissolve the coagulation; the liquors, etc., that preserve blood; the liquors that blood may admit from aliments; the spontaneous or natural analysis into a serous or a fibrous part; the quantities of the serous and fibrous parts; the differences between the serous and the red parts; the analysis of blood and fruit of its spirit; the volatile salt of blood and its figures; the phlegm of distilled human blood; the two oils; the fixed salt; the *terra damnata*; the proportion of the differing substances chemically obtained from blood; the fermentation and putrefaction of blood and its phenomena; the mechanical uses of blood, as in husbandry; the chemical uses; the medicinal uses; the difference between human blood as it is found in persons differently constituted and circumstanced, as men, women when menstruous and when not, children, Moors, etc.; and the affinity and difference between the blood of men and that of divers animals, as quadrupeds, birds, fishes, and sanguineous insects.

It is singular to us now living to note how readily Mr. Boyle obtained human blood for his inquiries. Bleeding was so common a process in his day that he had only to go to the surgeons' shops to obtain fresh blood from the subject, as we might now go to a hairdresser's to obtain specimens of beard or of hair. He seems to have been quite unobservant of any risk or danger from the all but universal practice of blood-letting. The facility of obtaining the blood under these circumstances rendered his experiments easy. Let me name a few of the conclusions at which he arrived.

He endeavoured to ascertain the specific gravity of the blood, taking water as the standard, and assumed it to be heavier by about the twenty-fifth part of its own weight. He was most particular in his inquiries on what he named the "spirit" of the blood, a substance he obtained by distillation together with a volatile salt. The spirit was obtained by distillation of blood drawn from persons that "parted with it out of custom," sick persons being unfit for his purpose. The spirit, so called, was possibly ammonia in solution of water, a fact he himself came to believe, since he queries whether the volatile salt is anything more than the spirit in a dry form. There is, at the same time, some difficulty in knowing what the liquid really was. He does not give us sufficient details as to the mode of distillation, and he informs us that the spirit was odourless, tasteless, colourless, and transparent, a description that in regard to odour and taste does not tally with ammonia. It was alkaline, had strong powers of dissolution, made copper of a beautiful azure colour, dissolved zinc easily, and rendered dark blood clot florid, like a fine scarlet. It neutralised acids in some instances, as with lemon juice, causing effervescence, and it acted as a splendid preservative of animal tissues. The great point on which he dwelt in respect to the spirit of blood was its medicinal virtue, both externally applied and internally administered. He presumed it to be a rational remedy in hysteria, pleurisy, coughs, epilepsy, apoplexy, jaundice, and many other diseases, including even confirmed phthisis.

So much for what can be said here about the natural history of the human blood. Microscopical observations there are none of moment, chemical and physical

any number, some good, some worthless, all original. One other observation must not be omitted. Boyle found that the urine of a man made a good invisible ink, and thinking of the analogy between urine and the serum of the blood, he traced with serum held in a pen some characters on a white piece of paper, and having suffered them to dry on, he held the unwritten side of the paper to dry over the flame of a candle, by which means the letters appeared, though not of an inky blackness, of a colour dark enough to be easily legible.

MEDICINAL EXPERIMENTS.

Under the head of medicinal experiments, or a collection of choice and safe remedies, Boyle gives a number of receipts at some of which we cannot but smile. He states very modestly that medicine is not his profession, but he hopes his collection will not incur the displeasure of charitable persons, even though they be physicians. Some of the receipts are nothing more nor less than disgusting, although quite in accord with the practice of the day. Some are useful. For staunching of blood he prescribes dried lycoperdon; for hæmorrhoids he recommends application of balsam of sulphur made with turpentine; for a sprain he orders wheat bran saturated with the strongest vinegar to be applied to the part affected as a poultice, and to be renewed when it ceases to be moist; for gout he holds by sarsaparilla; for itch he describes a wash of lime water in which mercury tied up in a linen bag has been suspended; for apoplexy he commends dry cupping at the base of the neck and at each of the shoulders; as a local application for gout he recommends one part of sal ammoniac dissolved in three parts of spirits of wine, really a very good lotion; for

ague he has faith in rue ; for hysteria he gives the palm to hartshorn. But here I must stop, for over a hundred and fifty formulas, more or less passable, more or less loathsome, more or less useful, are before me.

VARIOUS NOTES.

The chapter about Vitiated Sight is of moment as showing its author's knowledge of colour blindness, of distension of the eyeballs, and of congenital cataract. In the essay entitled "*Medicina Hydrostatica*," he discourses at length on the hydrostatical examinations of medicines. On "*Salubrity and Insalubrity of the air*" he dwells on the action of cosmical gases let loose into the atmospheric sea. In a treatise on the "*Corpuscular Philosophy*," he discusses the reasonableness of specific medicines to that philosophy; and in an elaborate chapter on the "*Porosity of Animal Bodies*," he explains how various substances may enter the body by the skin, so that mercury may actually be precipitated in its elementary state within the cavities of bones.

Eighty-six pages of large quarto size in double columns of small type are demanded for the index of the works of Robert Boyle. In the whole range of scientific literature there is no one half so prolific as this writer. The index itself is a study for many weeks. I, like a gardener, have merely roamed through the domain of his written mind, and culled here and there a flower, plucked from a bed or forest of luxuriance and beauty. Even in subjects purely medical my difficulty has been in selection. Five years devoted to the contemplation of these works would give but an inadequate appreciation of their greatness, their usefulness, their simplicity, and their truthfulness. I would I had leisure to learn from them more and more.

TO THE END OF LIFE.

Before touching on the works of Robert Boyle I briefly sketched his life up to the middle period of it. Let me now, as briefly, resume the narrative and follow him to the close of his brilliant but comparatively short career.

In the time of the great wars, and during the short peace of the Commonwealth, he resided chiefly at Oxford and Stalbridge, retaining the friendships of Willis, Nathaniel Highmore, Wallis, Ward, probably Harvey, Wren, and the best minds of his age. For a time he visited Ireland to look after his estates there, returning to Oxford as early as he could. He was throughout a consistent Royalist, but he seems to have avoided all part in politics. On the Restoration he was received with much favour by the King (Charles II.) and his most powerful ministers, and settling down in London an unmarried man, he took residence in the house of Lady Ranelagh, his sister, in Pall Mall, then a fashionable quarter. He aided greatly in the foundation of the Royal Society, and was elected to the council of that body, to which he was a voluminous and invaluable contributor of papers and experimental demonstrations. He had, of course, his enemies, and amongst these Dean Swift, who nevertheless was probably indebted to him for the bright suggestion of *Gulliver's Travels*, but he bore all criticism with patience and unfaltering dignity. One Valentine Greatracks, a charlatan, who pretended to possess the power of curing diseases by stroking—a kind of mesmerism, as we should designate it—was forced on his attention by Henry Stubbe, but he was not to be imposed upon, and in an admirable reply he let Stubbe

that, gentle as he was by nature, he knew true from false philosophy. Through the storms of the Royal Society he likewise sailed without censure ; and, as we have seen, he wrote freely about physic without incurring the wrath of the keen-eyed and ever-jealous College of Physicians.

About 1689 his health began to suffer ; and foreseeing that he had much to do and finish with but little time at his disposal, he issued a kind of card or notice in which he excused himself to callers, and put over his door an announcement to that effect. He also made his will, and settled all his private affairs, working on under a form of paralysis that was day by day more and more distressful. To his anxieties for himself, such as they were, the illness of his beloved sister, Sophronia, as he styled her, with whom he had lived in such homely communion, added its penalty ; and she dying on December 23rd, 1691, he followed her on the 30th of the same month, aged sixty-four years. Boyle was buried in the famous church of St. Martin's-in-the-Fields on January 7th, 1692, and in the sermon preached on his death Gilbert Burnet, Bishop of Salisbury, declared of him, "His design was only to find out nature, to see into what principles things might be resolved, and to prepare good medicaments for the bodies of men."

So be it the design of all men who would follow philosophy as their guiding star.

IDIOSYNCRASY AND ALCOHOL.

THEY who oppose total abstinence say that abstainers have an *idiosyncrasy* for keeping up their strength under abstinence. I am constantly told this, and am adduced as a man who won't see that, although I can keep strong on water—that is the way it is put—other people cannot. Well, but I have observed the late Cardinal Manning, and the Bishop of London, and Dr. Nathan Davis, of Chicago—who remembers that great city as a small village—and numbers more of active men all keeping up their strength on the same system; I also see nationalities of strongest men keeping up their strength under the same system; and if I turn from our own earth-mates to those of the lower creation, what are the wonders I behold? How poor is human strength to that of an elephant! A whale by one movement could cast twenty men stronger than Samson himself into the air; a pigeon would easily go ten miles to one accomplished by a man on foot; and the racehorse;—but I need not push the point further, except to intimate that these competitors in physical powers are all working on the total abstinence system, and that if we wished to hurry them down in physical power it would be by initiating them all into the moderate use of wine, spirits, or malt liquor.

Is there an idiosyncrasy in this matter of alcohol and its effects on animal life? Certainly. An idiosyncrasy is a peculiar condition of constitution by which one animal body is specially influenced by a certain thing or by certain things which do not affect the natural organisations of life and living action. Drinkers of alcohol, moderate or immoderate, are affected by a special idiosyncrasy in that they have a liking for one particular thing which gives to them a particular organisation, under which, like Oliver Twist, they "ask for more," and getting more, stand sometimes at peril, except always on guard, and then even in danger. We have heard it said recently that alcohol is no more a source of danger than fire. The analogy would be good, excellent, if we could swallow fire, and if, having swallowed it, the swallower cried for more until he were slowly or quickly consumed. But here is the difference. A man cannot swallow fire, and he can swallow alcohol, which latter being swallowed, he does cry for more, until he is slowly or quickly consumed. So the ancient fable compared the alcoholic fire to something that slowly burned the vital organs. This is all the difference in the world. The burned child dreads the fire; the burned alcoholic courts the slower alcoholic burning.

This simile of fire, although physiologically it is but a simile, how true it is. If we could divide all living things into two sections, the infinitesimal section in the world of life that takes in alcohol and the mighty section that takes in none of it, and if we could see through the two as transparencies, how striking and instructive the picture would be. The alcoholic section would indeed be seen to burn most freely, until we came to those who, overwhelmed by the excess they

have consumed, were all but extinguished and burnt out. The moderates, they who assume to pity and despise the intemperate, would shine out with a light peculiar to themselves, yet showing by flickering intervals how near their temperance edged on intemperance, and how imperceptibly to them the phenomena of temperate intemperance might be displayed. Practically they would all have taken the alcoholic fire. One would show how the alcohol brightened him up when he had "run down"; another how it warmed him; a third, a little too hazardous for once, how it gave him "hot coppers"; while a fourth would illustrate the fixed glow of the fire on his visage. Yet they might all be moderate men, distinct entirely from the dark, leaden, helpless collection who, through the moderate path, land themselves into the living death. If we watched them minutely, we should see, too, under the brilliancy of their superior fire, that their pulses were quicker than those of their fellows who let their watery bodies, made by nature to work on water, depend for motion on that neutral and potent agent alone; we should observe that their brilliant light day by day went somewhat quickly and quietly down, unless the sustainer of it were more and more frequently renewed; we should observe how for a season they were blithe and gay, rejoicing in the renewal of their youth in each cup they consumed we should hear them boasting of their strength of mind and limb, and should not wonder at their contemptuous raillery on ourselves, our tenets, and our practices. Let us, moreover, be fair to them; let us admit that if they are not such steady lights as we are they surpass us in temporary brilliancy. I, for one, am quite ready to admit my own dulness by the side of

them ; let us all admit the same, and allow the truth of what they contend for. They speak as they feel, and what else can they do ? Let us believe them, for they on their side speak truly. The question with us is why we do not follow in their brilliancy. Why not set up their idiosyncrasy ; why not become imbued with their fanaticism ; why not leave the lower and grovelling ranks of the creation at large, acknowledge that " wine's the soul of man below," and from our common mortality be made " half divine " ? What fools we must be to grovel as we do ! What cowards we must be not to feel that, raised to such brilliancy, we can reach whatever height of brilliancy we like, and without danger stop there !

Well, we have a reason for our obduracy, nay many reasons, which, dull as they may be, possess us. Let us examine a few of them. (1) We have trust in nature, and belief that we are not wiser than she is in regard to her designs ; if she has made all the phenomena of animal life to work by water power, and has supplied all the conditions and appliances for such work, we cannot help it ; it is the divine will, and let that will be done. (2) We admit that man can change the order of nature by an agent in which there is a new and strong power ; we admit that he can by the persistent use of this agent introduce a new kind of life, and, if he knew how, might extend that new life to all life, to fish of the sea, to fowls of the air, to beasts of the field, to creeping things, as well as to mankind, male and female, through all phases from the cradle to the grave—truly a wonderful art ! But follow it up. What if all living things were transformed by it into moderate drinkers, with the usual contingent of drunkards from amongst the weaker of them ? What

if all creation were made half divine instead of a few men and women specially blessed? What if the elephant were endowed by it with more strength and vitality and brightness of intellect? It is a sagacious creature and powerful; it might restrain itself to what was good for itself even better than some men. How much more sagacious and powerful would it be if it were made "half divine?" How useful would it be so super-endowed? We admit the effects of alcohol; but, in our judgment, all creation works better without it, and as man merely belongs to all creation, he too comes under the rule of creation, and is in like manner better without it. All creation does well without it; why should man be an exception? (3) Looking carefully at the brilliant human brotherhood that supports the "diffusion of useful alcohol" and picks up its delightful inspiration from that power, we are not entirely satisfied with the results. With the debris of that brotherhood, with its murderers, madmen, paupers, paralysed and otherwise death-stricken, we are so profoundly dissatisfied that we would be led to close up the sources of such gigantic evils, even if some considerable good came out of the same sources; but we are not convinced about any good. "The diffusion of useful alcohol" is, we think, not proved, diffuse it ever so lightly. The quickened throb of life which changes the brilliant and exceptional community of temperate men can only be sustained by repeating the cause, and repetition is a fearful and sure degenerator. Repetition soon becomes automatic, and becomes rhythmical, and to automatism and rhythm working abnormally together there is only one end, unnatural death. To automatic rhythm we succumb easily enough without alcohol. The heart of man continues to beat from

birth to death, without his consciousness, by no other regulation than automatic rhythm. The heart asks for blood, and getting it, goes on in its accustomed course. This is a natural act; a similar act may be acquired, and it is acquired in those who ask for alcohol and feel they cannot get on so well without it as with it. Imperceptibly they become the temperate intemperate. They may be "half divine" compared with us, but it is a queer divinity. The very difficulties felt in coming back from the artificial automatic rhythm to the natural are proofs of the serious nature of the aberration. They who feel the want at particular hours, in particular quantities, and under particular circumstances, soon find that the more brilliant they become the more they want the cause of brilliancy. The want supplied brings its physical cares. The heart beats more quickly to keep the fuel of the body in motion, the breathing is quickened to keep the animal fire alight, and the alcohol, assiduously cumulative, especially in the membranes and in the nervous centres, is being laid by until at last what may justly be called an alcoholic organisation is instituted, in which the elastic tissues lose their elasticity, and in which changes of tissue degeneration proceed, slowly or rapidly, as surely as the earth proceeds in its course round the sun. We need not dispute the greater brilliancy of the living organisation modified by alcohol. It has its own seasons, its rise, its status, its decline. Our argument is simply that we do not ourselves wish to purchase the brilliancy at the cost it entails. We do not court a new automatic rhythm; we are water engines by natural law, as all other living things are, and we feel it best to remain as nature has ordained us to be. (4) In all sincerity we do not

see that by our plan we lose anything of value which the moderate drinker gains. We are emphatically free of his dangers : of coming, without knowing it, under the automatic rhythm of alcohol ; of becoming temperately intemperate ; of becoming absolutely intemperate. These, at all events, are advantages. Neither do we feel that by remaining, under the wisdom and government of nature, as water engines, we are vital engines of lesser power in a physical point of view, than we should be if we let alcohol diffuse with the water we take into our organisations. The balance of evidence as to strength, endurance, and precision lies, in fact, on our side of the animal creation ; and if it be said of us that, in the human family, we bear the pale face, we may fairly retort that if colour be sign of strength, and white be pitted against red, then the white face is the strongest, as it is the most natural. To blossom like the red rose and darken like the mulberry is not, we think, the outward and visible sign of inward strength, physical or mental. (5) We know, and all who are candid know, of a truth that, amongst those who abstain from alcohol, there is a steady vital activity which does not pertain to those who believe in the dogma of the "diffusion of useful alcohol." The so-called temperate are imperceptibly intemperate in so many instances, it is impossible, we believe, to say where temperance ends and where intemperance begins. If we want to insure a life, we prefer the staunch abstainer to the doubtful temperate ; if we want to insure from accident, we do the same ; if we want to have men to whom we would entrust some great responsibility, the driving of a train, the supervision of fire, the protection of property, the tending of the sick, any post where there is most risk of failure,—

we seek those—and the greater the risk the greater the search—who are absolutely safe from the artificial, self-inflicted, automatic rhythm. The abstainer's life may be slow, but it is sure. He is lion-hearted as the lion is, and the lion, it is a fact beyond dispute, is a life abstainer. Balancing, therefore, the absolutely temperate with the temperate intemperate, and giving over the absolutely to intemperate those from whom they spring, we incline to forfeit brilliancy in favour of solidity of life, and to be one with nature rather than two with alcohol. In plain words, we disclaim alcohol as being in any sense a part of the natural scheme of healthy and useful vitality. If it had been intended for that end, we should not have to manufacture it; it would have been provided for us, distilled for us, delivered to us on the same gigantic scale as water is provided in the ocean-beds, is distilled by the sun, is condensed by the everlasting hills, and is carried to us and to every creature of life in river, lake, rivulet, and spring. In a word, we see that we are water engines all, and that the Master of life is the Engineer in whose unapproachable wisdom we can well afford to confide.

ORIGINAL RESEARCH, EXPERIMENTAL
AND INDUCTIVE.

"Dire n'est rien ; faire est tout."—RENAN.

COAGULATION AND OPACITY.

IT will be remembered that in the article on the diaphanous test of death commented on in last ASCLEPIAD the fact was shown that in a dead person the edges of the fingers transmitted light, as is commonly seen in the living subject. It will also be remembered that the blood in the same dead body remained fluid ; and the question arose whether the fluidity of the blood accounted for the diaphanous condition of the fingers. In order to put this question to the test, I have, during the past quarter, performed a series of experiments bearing on the subject, and, as far as I can see at this moment, they elucidate fully what required to be learned.

Having obtained a portion of defibrinated sheep's blood, I allowed it to stand until it became very dark, but not offensive. I then over glass tubes, representing fingers, stretched fine wash-leather, and immersed them in the blood, leaving them to saturate in the fluid. When they were quite saturated I removed them from the blood, dried them with a soft cloth and bibulous paper on their outer surface, and putting them together as fingers are, held them up to a very strong light proceeding from a powerful lens. At the points where these

artificial fingers came near together, light passed through, the red colour showing precisely as it does in the living fingers.

This matter settled, I proceeded to set up coagulation of the blood absorbed and held by the leather skin. I tried first to produce the coagulation by exposure to radiant heat, but was not so clearly successful as I could wish. Then I immersed the fingers in heated water at 160° F., under which the skin somewhat shrivelled, and another bad effect followed. Finally, I made an acid solution by adding one part of nitric acid to four of water at the ordinary temperature. The result left nothing to be desired. After a few minutes of exposure to the acid solution the blood was coagulated throughout, and the surface, as is common under nitric acid, was bleached, so that when the tubes were taken out of the acid solution and dried they very well resembled fingers, the skin appearing flesh-like in colour. Now, on being held up to the same degree of light as before, not a trace of red light nor of any light could be seen. There was just the same result as when the dead fingers of the body in which the blood has coagulated are subjected to the same test of light.

We may take it therefore as corrective of the diaphanous proof of death that when in any inquiry the blood in the veins remains fluid, as will often be the case in cold weather, the edges of the fingers will transmit light, though the body be dead.

The effect of coagulation of fibrinous and albuminous structures is to prevent the transmission of light through them. We see this in the fibrinous clot and in solutions of albumen. We do not see it marked in the same degree, however, in gelatinous coagulations or solidifications. What coagulation does to

interfere with the passage of light in the first-named class of substances is open to a further inquiry, which may perhaps throw light on the molecular change which constitutes the process, and gives it its name.

ON THE USES OF NITROGEN IN ATMOSPHERIC AIR.

EXPERIMENTS which I and others have elsewhere related show clearly that an animal will die if it be placed in pure oxygen; but it is worthy of remark that while oxygen in the undiluted form reduces life, because it cannot enter into combination at temperatures below 30° F., the same oxygen unadmixed with any other gas, if distributed and brought into active motion *by heat*, will destroy, ultimately, by just the opposite process, that is to say, by setting up too active a combustion. Thus if our atmosphere were composed entirely of oxygen, and if the same character of seasons could prevail, and the same variations of heat and cold could prevail, all life would be impossible. At the poles the oxygen would be so condensed that combustion of the blood would be prevented. At the equator the combustion would be so rapid that, according to our present conformation, we could not receive sufficient food to sustain the combustion. In the temperate zones, when the thermometer was below 30° F. we should die as from want of air; and when the temperature was above 80° F. we should die as from want of food.

All these points admit of demonstration, the time being given, in warm-blooded animals living in pure oxygen gas at different temperatures. I cannot detail this in the present paper, but I have named the general facts in order to point out what I think is a new truth in respect to the

uses of nitrogen to man. It is common to speak of nitrogen as the mere mechanical diluent of oxygen in the air. I understand it as more than this. It is the grand equaliser of heat. When the day is cold the loss of heat from the atmosphere is sustained as four to one by the negative nitrogen, and when the day is hot the increase of heat in the atmosphere is sustained as four to one by the negative nitrogen. Thus the oxygen, although it undergoes physical change under great variations of heat and cold, although it is actually more condensed at the poles than at the equator, is never so much disturbed but that it can sustain a certain degree of life.

By the presence of nitrogen, equalising the distribution of heat over the whole surface of the earth, the fluctuations in the activity of oxygen on man are kept within given limits. But there are fluctuations, notwithstanding, sufficient to lead to definite physiological phenomena affecting and influencing, not individuals only, but races of men, and by virtue of which the body is enabled to live in various parts of the earth under the most variable conditions.

If I take an animal living in the ordinary temperature, and place it, not in cold oxygen, but in cold common air, at 30° F.; if I feed it well and cover its body closely; if, in fact, I place it in the condition of a well-fed Esquimaux, I find that the animal will want to eat largely, will begin to make an excess of carbonic acid, and if fed as at 60° Fahr., will commence to waste. The reason for this series of events is that oxygen is abundant in the air, and at the same time is sufficiently diluted to be able to combine with the blood. And the result is a greater production of primary force by which the animal is enabled, when well fed, to sustain the effects of the surrounding cold.

If from this extreme degree of cold I move the animal

to a temperature, say of 70° F., still supplying it with common air, I find, if the food be kept up, and all else be equal, the animal ceases to crave so much for food, produces less carbonic acid, and with a decreased waste tends to grow fat. The reason for this series of events is that the oxygen, diluted for ready combination, does not meet the blood with the same degree of absorption; and the result is that the animal, which in the warmer medium does not require so free a production of heat, produces less.

But if, in repeating these experiments, I use pure oxygen instead of common air, the animal in the low temperature will want no food, will make a minimum of carbonic acid, and will die from cold; while the animal in the higher temperature will eat ravenously, get very hot, produce excess of carbonic acid, and, if not over-sustained with food, will die from waste. The differences in the results of these experiments, as compared with those related before, are due to the absence of the equalising nitrogen, which resists, in the common air, the excessive action both of heat and of cold.

COTEMPORARY PRACTICE AND LITERATURE.

"Every physician will and ought to make observations from his own experience; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together."—FRIEDS.

WATER FILTRATION, ARTIFICIAL AND NATURAL, AND CHOLERA.

MR. ALBERT J. A. BALL, B.A., of the Local Government Board, has translated a paper on the above subject, written by Professor R. Koch. The author of this paper takes the same view as that which has been held by some of our own epidemiologists, notably Chadwick, Simon, and Greenhow, namely, that, while the cause of cholera—the agent by which it is diffused—may, and does, in many instances find in water a suitable means of transit, it may be conducted and communicated by other methods, as by direct communication from person to person. Koch calls those who consider water to be the exclusive conductor "water fanatics" or "water theorists," but he does not tell us who these fanatic or theorists are or were. He considers that in great outbreaks of cholera the "chief importance is to be attributed to transmission, by indirect and various agencies, of the 'cholera germs,'" and that water is one of the most important amongst these, by which we must suppose that fanaticism and theory have been transferred from the water to the germ. Some illustrations of the water theory of communication

are given which remarkably support the late Dr. Snow's almost forgotten or ignored facts and the views he advanced on them in his famous volume on cholera and still more ably in his remarkable oration on "Continuous Molecular Changes." But the gist of the paper now under notice refers to the subject of water for potable use, and specially to water filtration. The author presses upon us the common-sense and the commonly accepted view—in England, at least—that surface water, however thoroughly filtered, should never be used, if it be possible to avoid using it, but that the supply for our populations should be subsoil water, which, he thinks, gives "absolute security with respect to the danger of infection, if it can be obtained in sufficient quantity, and is not objected to on account of its hardness or chemical characteristics." It should be preferred under all circumstances to surface water, soil filtration being superior to any practicable artificial process of the kind.

THE ALLEGED INCREASE OF CANCER.

FOR some months past the medical and general press has been anxiously repeating the statement that the disease cancer is on the increase, and that especially amongst the better-provided classes of our community, the returns of death seeming to favour this view. Dr. News-holme and Mr. George King have, therefore, made a special study on this point from the returns of the Registrars-General of England, Ireland, and Scotland, and have compared the death rates with other data obtained from the experience of the Scottish Widows' Fund and the official cancer returns for the city of

Frankfort-on-the-Main. The work of the investigators has been brought before the Royal Society, and the results may be considered as a satisfactory explanation that may prove of a soothing character to those who are filled with fears of the increase of the most intractable of human maladies. The grand conclusion arrived at is that the supposed increase in cancer is only apparent, and is due to improvements in diagnosis and more careful certification of the causes of death. In so far as the present statistics go, the view thus expressed appears to be based on sound and excellent reasoning. The one fault, if fault it can be called, is the limited scope of the inquiry, a necessarily limited fault that will be removed by further research as time yields the figures for the calculation.

POSTERIOR URETHRITIS IN CHRONIC GONORRHOEA.

PRACTICAL surgeons will be interested in reading the conclusions of Dr. Bransford Lewis, of St. Louis—delivered before the June meeting of “Genito-urinary Surgeons”—on the frequency of posterior urethritis and its influence in the production of chronic gonorrhœas. Lewis looks on the posterior infection not as a complication, according to the notion hitherto accepted regarding it, but as a natural feature, occurring with such unfailing regularity that an observer watching gonorrhœal cases carefully and critically must see a great many of them before he would meet with a single case that remained free from the so-called complication. The time and mode of onset of the posterior inflammation is of moment. Instead of the inflammation progressing slowly backwards over the

urethral mucous membrane, and reaching the posterior urethra in the second or third week, as is commonly taught, it reaches the posterior urethra in most instances in the first active week of the disease, a view which favours the theory of Harteloup that the mode of infection is through the lymphatics rather than by continuity over the mucous surface. Amongst the author's final conclusions we find the suggestion that posterior urethritis, by reason of its anatomical seclusion and inaccessibility to ordinarily prescribed treatment, offers the best explanation of prolongation, or repeated recurrence, of the disease. Direct topical treatment of the posterior urethra is, therefore, in the great majority of cases, necessary.

SATURNINE APOPLEXY.

IN the *Archives Générales de Médecine* for January, 1892, will be found, at p. 90, a report of a case of cerebral hæmorrhage with effusion of blood into the ventricles, ending in the death of a patient, who was under intoxication from lead. The case occurred at the "Hospital Saint Antoine," in the clinique of Dr. M. Hanot, and is reported on by M. A. Létienne (*Interne du Service*). I refer to it as an illustration of the extensive degeneration* that may occur from saturnine intoxication, and as showing, what has not been previously enforced, apoplexy as the immediate cause of death in the course of saturnine disease. Drs. Stewart, Blyth, and Oliver have given us many valuable facts regarding saturnine poisoning, but this termination has been so rarely noticed, it claims both for medical and medico-legal practice particular regard.

"A PRACTICAL HANDBOOK ON MIDWIFERY."

DR. NICOL HAULTAIN, who writes this little volume (Scientific Press), professes to do no more than offer practical help to the busy practitioner, and at the same time place before the student, succinctly, the chief points of scientific importance in the study of obstetrics. In this respect he keeps strictly to his word, and a practitioner or student could not wish to have ready, in emergency, a more concise or sensible guide. There are thirty sections of the work under which obstetrical rules are given in a few words that are easily read and understood, with sufficient reason to make the advice rendered plain and simple. There are also seventeen illustrations, which play an important part in the more complicated descriptions. The sections devoted to "Complicated Labour" are specially practical, and the one detailing the operation of cæsarian section is just such as the operator would like to peruse before he commences his task. Dr. Haultain thinks—and most of us will agree with him—"that it is more than probable cæsarian section will supersede to a great extent, if not entirely, mutilation of the fœtus in all forms." The book, adapted, by the way, for the pocket, is truly an Obstetrician's companion.

SUICIDE AND INSANITY.

DR. S. A. K. STRAHAN publishes (Swan Sonnenschein & Co.) a physiological and social study on the subject of suicide and insanity. The work is opportune at a time when suicide in this country is presented in so frequent a manner. In speaking of the increase of suicide, the author tells

us that 2,459 suicides were committed in England and Wales during the year 1891, but he adds "that in all human probability not more than half, even if so many, of the suicides which are annually committed in the country are ever discovered or recorded." He also shows to us that of late years the number of suicides has risen steadily, a rise, in fact, amounting to an increase of over 75 per cent. in twenty-two years, and that increase of population is not responsible for this difference is shown by the fact that, while the rate was only 61 to the million persons living in 1867, it had attained 82 per million in 1888, an increase of more than a third within the twenty-one years. This steady and rapid increase is also common to the whole civilised world.

The cause of this alarming increase of self-destruction lies, the author thinks, in "that degenerate condition which is the constant product of civilisation." Drunkenness, gluttony, and all other vicious excesses lead to degenerative changes in the body that cause suicide and such other allied evils as insanity, idiocy, habitual drunkenness, epilepsy, and allied crime. Intemperance, he thinks, adds something to suicide, and the increased struggle for life something; but the grand increase, he considers, is due to "hereditary transmission." He thus classes and classifies the phenomenon of suicide with those hereditary diseases, from cancer to congenital malformation, which we know to be transmitted from parent to offspring. In this, as in most other particulars and arguments in this able volume, I should largely agree; and I commend the book heartily to social scholars as well as professional, with the certainty that it will be read throughout with unflagging interest.

"*ESSENTIALS OF CHEMICAL PHYSIOLOGY.*"

IN the opening passage of this book on *Essentials of Chemical Physiology*, by Dr. W. D. Haliburton (Longmans, Green & Co.), the author states that it is written with the object of supplying the student with directions for examining practically the most important of the subjects included under the heading "Chemical Physiology, or Physiological Chemistry." At the same time, it is intended to serve as an elementary text-book of the subject. The introductory chapter concludes with a list of set reagents for work, with tables of weights and measures and with symbols and atomic weights of the principal elements. After this, in succeeding chapters, follow series of lessons on analyses and examinations of all the chief secretions and parts of the body. The rules for the examination of urine are very concise and exact, and the double purpose of making the work a text-book and a guide-book is ably sustained throughout.

"*DISEASES OF THE LUNGS AND PLEURÆ, INCLUDING CONSUMPTION.*"

THE fourth edition of the work under the above title, by Dr. Douglas Powell (Messrs. Longmans, Green & Co.), indicates how well known and how widely appreciated it has become. The new edition is a distinct advancement on the last, although revision is the great distinguishing feature. Dr. Powell has shown a wise discretion and a sound judgment in not forcing at all points the microbic view of phthisis, as if the understanding of that disease depended altogether on the now popular, but perhaps already doomed, hypothesis, and admitted

of no other exposition. He says very truly that the microbic theory, if taken seriously by the practitioner, cannot fail to be most despondently regarded by the patient, a fact which every careful and unprejudiced physician must constantly realise. He holds tenaciously, but not too tenaciously, the view of hereditary influence in respect to the occurrence of phthisis in those who are attacked by it, and he is most strongly imbued with the importance of prophylactic measures in treatment.

A valuable and interesting chapter is added in this edition on "Actinomycoses of the Lung." The post-mortem records of the Brompton Hospital, with one single exception which the author refers to, show no example of the affection. The case, therefore, in its singleness was well worthy of being permanently described as it is on pp. 302—320 of this volume. The patient was a boy aged nine, and his parents, who seemed free of physical tendency, dated his symptoms from a blow, received at school, behind the ear. The clinical features of the case, which are all minutely detailed, were those of empyema; but after exploration and removal of a portion of the sixth rib, the existence of some degenerating growth was presumed, and turned out to be correct, evidence being abundantly supplied in the end showing the disease to be a local inflammatory reaction of the tissues, chiefly in the lower two-thirds of the pleural cavity, infiltrating the chest-wall and fungating through the cutaneous surface; the whole including extended mischief induced by the introduction and growth of the vegetable fungus the actinomycetes.

Patient inquiry was made as to the origin of the disease, and when and how the germs of the fungus entered the respiratory passages. There was no evidence that the disease was in any way connected with

a bovine source, and there was nothing sufficient in family predisposition to account for what took place. So far, therefore, etiologically, rational explanation as to mode of infection fails.

I have picked out one section of Dr. Powell's work for notice because of its novelty and of the skill and candour with which it is presented; but all the rest is equally good.



THE "ANNUAL OF THE UNIVERSAL MEDICAL SCIENCES."

THE 1892 issue, in five volumes, of this annual is an event in medical literature. Dr. Sajous, the learned editor, has now moved to Paris, and has made that city the centre in which his divining-rod plays its part. With seventy associate editors and two hundred corresponding editors and collaborators, he has sufficient to fill the work and mind of the most ambitious editor the profession of medicine has ever produced; and when the fruits of his annual labours reach the shelves of the practitioner, the practitioner can only feel that he has a library before him that will barely be read through before another, still more advanced, will be in his possession. In brief, this collection will ere long, if it keeps up its reputation and its present mark, stand alone in its glory. What is now wanted, in respect to these volumes, is an annual commentary on the whole in a single volume, in which, in easy and very comprehensible language, there shall be depicted to the reader, void of all figures and references, the pith of the whole, with particular attention to every spark of really new light that may have scintillated, and which might be lost very easily in the mighty crowd of

details. It is impossible for the most painstaking editor of a department to single out, at the moment, any special advancement; it is his duty to present the whole truth that comes under his observation without fear or favour. When all is done and in print, he might be advantageously consulted as to what he considers best, but the critique altogether should be by one mind that has read the whole and made the very best of it visible for the perplexed and heavily engaged medical community.

BOOKS ON ANATOMY.

SEVERAL books on anatomy have recently appeared. I notice on my table the first and second parts of *Dissections Illustrated*, a graphic handbook for students of human anatomy, by Gordon Brodie, F.R.C.S. (Whittaker), the plates drawn and lithographed by Percy Highley. Mr. Brodie must indeed be congratulated on the success of his effort, and so must the student who is so fortunate as to take assistance from it. I have seen nothing more perfect than these plates, nothing terser than the letterpress.

The thirteenth edition of *Gray's "Anatomy,"* edited by T. Pickering Pick (Longmans), is now published. The editor points out that the book is specially planned so that it may be of use to the student who will hereafter have to apply his knowledge of anatomy to his practice of surgery. The editor also thanks Professor Pearson for assistance rendered in the preparation of drawings "made by this master in the art of dissection." It were superfluous to say a word in favour of *Gray's "Anatomy."* It has been a work

which, ever since it left the hand of its accomplished and short-lived author in 1658, has held its place as the leading text-book of its kind, the *vade mecum* of Erasmus Wilson in better form and more adapted to the surgical requirements of anatomical teaching. That it has run a course which even its author could not wish better goes without saying, and the new edition sustains the old right royally.

A Plea for Practical Work in Anatomy, by Thomas Cooke, F.R.C.S. (Longmans), is a pamphlet by one of the best teachers of anatomical science we have ever possessed. The dead body, as examined in our dissecting rooms, is the one standard we, as human anatomists, should abide by"; "But never before, it is believed, have men done so little thorough work in anatomy." The origin of the small treatise is embodied in these two truthful sentences.

STATISTICAL RETURNS OF THE LONDON TEMPERANCE
HOSPITAL.

INCE writing the notes on hospital statistics, pp. 265-9 of this ASCLEPIAD, the returns of the London Temperance Hospital, drawn up with much care by Dr. Wilde, the able registrar of the institution, have come to hand. The results are most satisfactory. The in-patients numbered 859, of which 549 were cured, 201 were relieved, and 54, or 6·2 per cent., died. "The total number of beds in use in the hospital is 86, about 10 patients to each bed during the year; the average weekly number of beds occupied, 70; the average stay of each in-patient, 29·1 days." The medical cases were 421; the surgical and ophthalmic, 438.

Tables giving all details of the cases of lobar pneumonia and broncho-pneumonia, of enteric fever and alcoholism, of special operations and ophthalmic operations, are printed. The cases of acute lobar pneumonia were 29, of which four died, two within twenty-four hours of admission. Deducting these, the mortality was at the rate of 7·4 per cent. These cases were complicated with pleurisy in five instances, and empyema in two, both of which recovered. "Of the four deaths, three were drunkards, admitted delirious and with both lungs implicated; one was an infant four months old." There were five cases of enteric fever, with recovery in every case. The six cases of alcoholism all recovered, although delirium and violent excitement were present in three of them. Two of the female cases came in suffering from alcoholic paraplegia, and both recovered. No alcohol was given after admission, milk being given for the first twenty-four hours. The cases of rheumatism were 47, with two deaths.

The surgical operations were many of them serious, but there were only five deaths amongst them.

The above is a brief review of the results in a few classes of cases, but many others of an important kind were also admitted, with results equally satisfactory. In no case was any alcohol prescribed on the medical side, and in one only on the surgical.

SOME GENERAL LITERARY NOTICES.

EVERAL works of a delightful general kind of literature ask for attention, and demand notices far longer, if not more appreciative, than can be afforded them in this place.

IN RE WALT WHITMAN.

Edited by his literary executors—H. L. Traubel, R. M. Bucke, and T. H. Harper—claims first attention. Dr. Bucke plays a conspicuous part in this editorial trio, and never did the distinguished author of *Man's Moral Nature* shine better. A close friend of the poet, he gives his record as manfully as sympathetically of the strange being whom some treated with coldest disrespect, others with scorn, others with an admiration amounting very, very nearly to idolatry. All scholars, whoever they may be, will, however, read this book about Walt Whitman from end to end even if for psychological curiosity alone. It is sometimes sad, sometimes ecstatic, oftentimes excentric, always, from whichever view, picturesque. "Leaves of Grass," the great poem of the singular poet, are the leaves of this book in a metaphorical sense, and they are always green. The history of the last illness of Whitman is graphically and candidly told by Dr. Daniel Longaker, who professionally attended the poet, and who, speaking of the post-mortem results, says that to medical ears at least it may seem strange that physicians of even average diagnostic skill should overlook a large pleural effusion, three and a half quarts of serous fluid in the left pleural cavity. There were two reasons for it: the first, a lack of complaint of pain; the second, respect to his own disinclination to be disturbed. "And," says the writer naively, "practically this failure of discovery made little difference, since it is doubtful if the removal of the fluid would have added much to his comfort or succeeded in prolonging life."

There are altogether thirty essays on Walt Whitman in this volume. Three of these are unique in being

favourable reviews of his own work by himself. One, by Colonel Robert G. Ingersoll, is memorable for its impassioned eloquence on "Liberty in Literature"; and another, entitled "Round the Table with Walt Whitman," is perfectly novel in its construction and matter. But the chapter which is most striking is on "Walt Whitman and the Cosmic Sense," by Dr. Bucke, in which that original scholar argues that his hero "had a mental function not possessed by ordinary men, and that that function is a form of consciousness transcending the common consciousness that we all have, and superadded to it." This new sense, he believes, is far from being isolated; it is a definite entity of the very first importance; he knows personally seven other men, all at present living, who have it, or have had it, in less marked development. It appears in individuals mostly of the male sex, men of good intellect, of high moral qualities, of superior physique. It shines about the time of life when the organism is at a high mark of excellence and efficiency, at the age of thirty to forty years; and the time will come when to be without the function in question will be a mark of inferiority parallel to the absence, at present, of the moral nature. Man in his progress from the lower, or lowest, to the highest development, attains first simple consciousness, then self-consciousness, and, finally, cosmic consciousness. Walt Whitman had attained to the highest of these developments.

Such is Dr. Bucke's theory. It is fair to say that at least one of his literary companions does not accept it; and it is fair to him to add his confession that for a fuller exposition he is preparing a special work. For this with due patience and eager expectation we must wait.

FROM OUR DEAD SELVES TO HIGHER THINGS.

In a former number of ASCLEPIAD, I had the pleasure of treating on a small book called *The Lord of Humanity*, written by Mr. Frederick James Gant, F.R.C.S. This author has now presented us with another small volume under the title given above (Nisbet & Co.), which may be considered in some sense as supplementary—or may it not be called complementary?—to the first. It aims at presenting higher things than mere man. It runs a tilt at Renan, arguing that his prose poetry dazzles the worshipper, but that in it the pinions of faith are wanting, so that the soul after reading it at last settles down into its own dead self. It is hoped that the readers of the present essay will pass more surely from death into life, and that the veil of Agnosticism may be rent in twain from the top to the bottom.

There are three parts to this book: (1) "Our Dead Selves"; (2) "To Higher Things"; (3) "Higher Things." The chapters of which each part is made up are brimful of interest, even to those who will or may look on them as scientific, religious, enthusiastic themes. There is a chapter, beginning at p. 75, entitled "Intellect Awakening," a very happy title, in which the singular admixture glanced at is well displayed. "The human mind," says Mr. Gant, "does far more than register the impressions received from the senses. The observation of the phenomena or appearances in nature, which may have done little or nothing to satisfy the yearning of the human heart, is followed by the intellectual act of analysing them." This is as if there were an antecedent human mind, and not as if the human mind were itself the very phenomena themselves received, fixed, and, in fact, the mind.

He is happier in his argument, although it has a

strong dash of Paley about it, in speaking of a designing intelligence. The works of nature, he urges, have their analogy in works of human art. "Experience," he declares, "testifies to the personal knowledge of design in all works which come from the hands of man, and he suggests, therefore, a similar rational explanation with relation to all the works of nature, which themselves bear marks of design in the endless combinations and transmutations of matter." In this argument Mr. Gant goes nearer probably than he intends to the strict scientific school of thought, and in regard to the relation of the works of man to those of nature and the unity of mind in both, for he seems to imply that man does not copy, but originates, as Nature herself does, as if he were one and the same power.

There is a great deal more in this chapter on which it would be pleasant to dwell, short passages here and there that deserve to be selected for illustration, as such a one as this: "In evolution mind is an evolver, and is itself evolved." Or again, "In evidence of constructive designing intelligence, let the student of nature concentrate his attention upon any particular part of a living being. What does he find? Careful examination discloses to him a definite construction or arrangement of constituent portions which co-operate to fulfil a definite function, a purpose for which the part is constructed, however far back the designer may be traced through intermediate agencies."

But it is necessary at this point to stop with congratulations to the author for having evolved a work which shows an individuality of a most distinct and unexpected kind, in which religion and science go hand in hand by a new condition of argument and suasion.



DR. PASCAL AS A MEDICAL STUDY.

THE journalists have seized M. Zola as if he were their exclusive property; but they cannot seize Dr. Pascal, because, creation as he is, he is exclusively ours. We confess, however, we would rather have M. Zola than Dr. Pascal, with whom, after reading his life and career from Alpha to Omega, we are not in sympathy. Pascal was not a great man in physic; he was barely a student, and what there was in him that should lead Vizetelly to retire from his labours of translation in order to hide his tears we cannot divine. As a medical experimentalist Pascal was as unfortunate as he was clumsy, and his hypotheses on life and death were as far from originality as from sound philosophy. If he were what he was by heredity, and the child he is supposed to have left be a necessary extension of the family, the sooner it shuffles off the mortal coil the better for humanity.

TO CORRESPONDENTS.

In order to prevent the frequent and troublesome mistake of addressing communications to the "Editor," it is necessary to repeat that everything published in the ASCLEPIAD is exclusively by the Author whose name is on the title-page.

Books, Reports, and Pamphlets intended for notice in the ASCLEPIAD should be addressed to the Author at his residence, 25, Manchester Square, London, W.

Names of Subscribers and other Matters of Business should be forwarded to Messrs. Longmans, Paternoster Row, London, E.C.

Advertisements, and all Communications respecting them, should be forwarded to Mr. A. P. Watt, 2, Paternoster Square, London, E.C.

The Author will be greatly obliged if senders of Newspapers and Magazines would kindly mark the passages to which they wish to direct his attention.

3rd
Quarter,



1893.



*THE RANGE OF MORTALITY FROM
ENTERIC FEVER.**

I HAVE been requested by the Board of Management of the London Temperance Hospital to prepare a report on the facts relating to the mortality of enteric fever in the wards of their Institution from its foundation in 1873 until the close of the year 1893. I was also asked to study comparatively the results of the treatment pursued in this hospital with the results of the treatment of the same disease generally, and to pay particular attention to the point whether in this Institution the omission of alcohol as a medicine has been favourable or unfavourable to the patients under treatment.

The request came to me three months ago, and in accepting it I thought that the labour implied could be carried out in two or three days; but as I got into the subject I found it so absorbing and curious, it led me far beyond the boundaries of an official report, and induced me to extend the inquiry into an essay or paper worthy of wider consideration, in which sense it is now submitted to the Profession of Physic.

* Paper read before the British Medical Temperance Society on Tuesday, February 27th, 1894.

THE NUMBER OF CASES, AND MORTALITY.

The number of cases of enteric fever admitted in the hospital, from its foundation in 1873 until December 31st, 1893, has been, all together, 144. Of these 144 cases, 25 have proved fatal; but out of the fatal number 4 were admitted moribund, and do not come in calculation. The mortality, therefore, has been 21 out of 144, or, under proper appreciation of the term "percentage," 14·5 per cent.

Before proceeding, I am anxious, without considering it a digression, to give to the term "percentage" its true position and value, so that serious misapprehension may not arise in the employment of the term and in the application of it to smaller numbers than an actual hundred. While writing this paper I have been told of an Institution in which in one year the death-rate from typhoid was 50 per cent. I might say thereupon, if I wanted boastingly to criticise any detail of treatment in that place which was opposed to my ideas, how bad the results were in it. The fact stated was misleading. Two cases of the disease were admitted, and one died—50 per cent. as stated.

Whether the mortality from enteric fever in the Temperance Hospital is above or below the mortality of the same disease generally could only be determined by comparing it with data of mortality on the largest scale obtainable, so as to avoid all accidental circumstances that might lead to a false conclusion. I have therefore, collected the data from large sources of supply.

I inquired, in the first place, naturally, as to the death-rates of the fever hospitals under the Asylum

THE RANGE OF MORTALITY FROM ENTERIC FEVER. 339

Board, London, and am indebted to my colleague, Dr. Collins, for the following table:—

TABLE SHOWING THE ADMISSIONS AND DEATHS OF ENTERIC FEVER PATIENTS, AND MORTALITY PER CENT., AT THE FEVER HOSPITALS OF THE METROPOLITAN ASYLUMS BOARD DURING EACH YEAR SINCE 1870.

(Extracted from the Registrar-General's Annual Summaries.)

Year.	Admissions.	Deaths.	Mortality per cent. of Patients treated in Managers' Hospitals.
1871 { (15 months to)	279	57	21·96
1872 { Dec. 31st, 1872)			
1873	381	56	15·13
1874	435	63	14·87
1875	299	78	24·68
1876	288	59	20·34
1877	372	79	22·93
1878	484	100	20·26
1879	385	74	19·73
1880	248	43	15·63
1881	415	86	21·47
1882	515	104	20·71
1883	486	74	15·64
1884	493	98	18·82
1885	220	36	15·82
1886	333	47	14·85
1887	441	61	14·59
1888	450	72	14·64
1889	290	41	15·15
1890	498	93	19·68
1891	755	106	14·52
1892	430	65	13·20
Totals	8,497	1,492	17·5

From this table it is shown that, during the years 1871-92 (twenty-two years), 8,497 cases of enteric fever have been admitted, out of which 1,492 have died—or 17·5 per cent., 3 per cent. above that of the Temperance Hospital.

In order to learn further particulars on the subject of mortality from enteric fever, from figures on a large scale, I gained, through the kindness of Dr. C. Kelly of Worthing, the facts of the mortality of the disease during the great epidemic in that place, which, commencing in or about May 18th, 1893, continued until

November. In this outbreak there were 1,318 persons attacked, 619 males and 699 females. Out of these were 170 deaths, 70 males and 100 females, yielding a percentage mortality of 12·8, or 1·7 lower than the rate of mortality in the London Temperance Hospital; a result that is not remarkable, taking into account the differences of locality, and, on the whole, the better-class community subjected to the disease in Worthing than in the very poor of the Metropolis.

A third series of figures bearing on this same subject are from the returns of Dr. E. W. Hope, the able Medical Officer of Health for Liverpool. In these tables Dr. Hope has supplied the relative mortality amongst patients treated at home and patients treated at hospitals. His data extend to various diseases, and his findings as to enteric fever are as follow. The mortality amongst cases of enteric fever in London hospitals is returned as averaging 13·2 per cent. for the year 1892; in Sheffield 14·3 per cent. for the same year; and in Liverpool, from a total of 950 patients admitted into hospital during 1891, 1892, and 1893, of 11·6 per cent.

In cases treated at home the mortality was much higher. In Sheffield, in 1892, it rose to 25 per cent., as compared with the 14·3 per cent. in hospital; while in Liverpool the home mortality during the years 1891, 1892, and 1893, out of 1702 patients treated, was 18·6 per cent. as against the 11·6 of hospital returns.

By comparison of the returns of the Temperance Hospital with the above, it appears that the London hospitals' mortality for the year 1892 was 1·3, that of Sheffield for the same year 00·2, and that of Liverpool for 3 years 2·9 per cent. lower than that of the twenty years' average of the Temperance Hospital; while the Home mortality in Sheffield in 1892 was 10·5, and in

Liverpool for 1891 2-3 was 3·1 per cent. above that average.

FLUCTUATIONS OF MORTALITIES.

In studying the Asylums Board records one striking fact stares us in the face, to which attention cannot be drawn too early. It is that the percentage of deaths undergoes the most remarkable fluctuation. For example, in the year 1875 the mortality from the disease, as indicated in the table, rose to the rate of 24·68 per cent. from 299 cases, whilst in 1885 it ran down to 15·82 per cent. from 220 cases, and in 1892 it fell to 13·20 per cent. in 430 cases. The variations are difficult to explain, and can only be explained on the supposition that different outbreaks have different degrees of fatality despite treatment, the range moving, in the whole period, from 24·68 to 13·20. Whatever the cause of the fluctuation may be, it is sufficient to make every prudent man careful in coming to conclusions as to details of treatment before he draws any sweeping conceits as to his own practical influence on mortalities; it will prevent him also from noticing any one cause as affecting the result, unless it be so phenomenal as to ride over everything else that has previously been recorded, and prove by constant repetition one constant effect for evil or for good.

An analysis of the records of enteric fever and of the mortality from it in the London Temperance Hospital affords, in a minor degree, similar evidence. From 1873 to 1885 the mortality was 8 deaths in 66 cases—that is, at the rate of 12·1 per cent.; from 1866 to the close of 1891 there were strictly 13 deaths in 60 cases, or at the rate of 21·6 per cent. In 1892 and 1893 there were 14 cases with 1 death, or at the rate of 7·1

per cent. In a sentence, there have been favourable and unfavourable periods so distinctly marked that by selecting from them cleverly a friendly critic could give the results, from brilliant evidence of success, whilst an unfriendly critic, resolute in forcing a prejudice, dear to him, could, by selection of another period, make the results obtained extremely indifferent.

In attempting to determine the cause of these curious fluctuations, both on a large and a small scale, we have to consider the activity of the fever; the influence of season; the constitutions of the persons affected; the state in which the patients were admitted into such institutions as a metropolitan hospital; and the stage of the disease on admission.

The last named consideration the state on admission is the most important. Into a public hospital we admit patients with enteric fever in all states and stages. Sometimes they come from long distances, after fatiguing journeys and exposure; they are usually in the ninth or tenth day of the affection, often later on still, and if they remain some forty-eight hours within our doors before they die, they are accepted, however bad the symptoms they may present, as additions to our table of mortality. There must, as a necessity, under such circumstances, be always a considerable mortality over which we have no control. In illustration, let me describe, in the briefest terms, a few typical cases out of the fatal ones that marked the middle of what I have called the unfortunate period, 1886-91.

In one case, a girl of fifteen, whose father, mother, and three sisters had died of consumption of the lungs, sank from sudden hæmorrhage, and the *post mortem* revealed not only double perforation of the

peritoneum, but enlargement of the mesenteric glands and numerous other organic lesions, which rendered recovery impossible. Another patient, a child of seven, was admitted suffering from diarrhœa, which continued until her death. She was retained 79 days, and at the *post mortem* was found to have adhesion of the intestines along the whole tract, with enlargement of the mesenteric glands, and purulent matter in both pleural cavities. There was no rash, and it is a question whether the case should be classified as one of enteric fever. A third patient had lobar pneumonia of both lungs, with the enteric symptoms. A fourth sank from perforation. A fifth, five days after admission, died from profuse hæmorrhage. A sixth died from perforation of intestine. A seventh, who died on the third day after admission, was a heavy drinker, had suffered from *delirium tremens*, from frequent attacks of hæmatemesis, and the day before admission had lost a pint of blood by the bowels, the *post mortem* revealing sloughing of intestines and cirrhosis of the liver, a case altogether doubtful as true enteric. An eighth died from perforation, the *post mortem* showing warty vegetation on the mitral valve. A ninth suffered from copious hæmorrhage on the day after admission, followed by peritonitis and persistent vomiting, with death on the seventh day, the temperature being subnormal all the time. A tenth sank from sudden perforation and peritonitis. An eleventh died on the twenty-sixth day, after developing right hemiplegia and aphasia.

An institution placed as this is, ready at any moment to receive the worst as well as the mildest cases of the disease, must, as a matter of course, take its chances, without any complaint at being so unfortunate at any time as to receive some who are fatally stricken.

OTHER COMPARISONS.

If from the comparison of the mortality of the Temperance Hospital with that of the large mortalities above considered we were to pass to comparisons on a smaller scale,—comparisons, that is to say, of individual hospitals in the metropolis,—we should still find every reason to be satisfied, but on this point I would not dwell.

If it had turned out that in the Temperance Hospital the number of deaths in proportion to the cases treated had been larger than that which had occurred in the hospitals of the Asylums Board, larger than that of Worthing, larger than that of London, Sheffield, and Liverpool, larger than that of the United Kingdom (16 per cent.), signally larger than that in any other hospital, or larger than in cases treated at home,—a question might be raised, and fairly raised, whether our method of treating the disease without alcohol had not contributed such an unfavourable result, and the minds of the members of the Board and of the medical staff of the Temperance Hospital might well be anxious. But we may rest assured that, so far from any calculable evidence being afforded to that view, the opposite view might be taken. I would not take an opposite view myself, because our data are insufficient. All calculations made on such a small number of cases are off the field, worthless, and merely fruitful of false inferences; so that the records of this hospital, extending to 144, should be rejected, were they much more favourable or unfavourable than they are, by all conscientious and responsible men. The most that can be said is that if the net result of 14·5 per cent. mortality in the small number of cases in the Temperance Hospital be compared with the net results, given above, from London,

Worthing, Sheffield, and Liverpool, including home as well as hospital treatment, the 14·5 of the hospital would stand against 15·2.

THE QUESTION OF ALCOHOL.

The question whether alcohol plays any part for good or evil in the treatment of enteric fever remains entirely *sub judice*.

In the *Glasgow Medical Journal* for February 1893 there is an admirable note on "Statistics of Enteric Fever," by Dr. Donald Macphail, in which he extracts from Reports, published in America, Australia, and Scotland, some most important facts bearing on the subject in hand. To some of these I must refer.

Dr. A. L. Mason publishes statistics of 676 cases with a death-rate of 10·4 per cent. altogether. But dividing these into thirteen series of fifty cases each, the death-rate varied in each series from 6 to 20 per cent. Dividing them into six series the death-rate ranged from 6 to 14 per cent., whereas three series of two hundred cases differed only 1 per cent.

Dr. F. E. Hare of Brisbane found that in three series of six hundred cases each the death-rate varied between 14·3 per cent. and 15·3; so that the conclusion is drawn that groups of six hundred are large enough to allow a comparison of the comparative effects of treatment.

Dr. Macphail gives the statistics himself of all cases of enteric fever admitted to Belvidere, Glasgow, during eight years. The cases numbered 2,334, and the death-rate over all was 11·3 per cent., the variations being from 7·01 to 15·6; to which fact he adds that this difference was not due to any difference in mode of treatment in the different years, the same principles being adhered to throughout and carried out consistently. The

variation, he says, "betrays a source of serious fallacy in observation upon the results of different modes of treatment of enteric fever. If it had happened that a physician had taken charge of this hospital at the end of the first year, and had introduced new methods of treatment and then compared *his* first year's results with those of his predecessor, the lowering of the death-rate by 3·6 per cent. would probably, and with apparent justice, have been attributed to the 'improved method.' Perseverance in the same course for two years more, with more rigorous attention to the details of his own method, would have apparently confirmed his results by bringing down the death-rate to less than half what it had been for the year previous to his entering upon office. Further observation during the next four years, however, would have disappointed him and staggered the faith of his disciples. It seems clear that observations on the results of treatment in enteric fever must extend over a very considerable period, and over a large number of cases, before very reliable general statements can be founded upon them."

So much from the observations and reasonings of Dr. Macphail, whose experience tallies well with my own. When I was a student, enteric fever was becoming fully recognised as a distinct malady; and the late Dr. Perry, of Glasgow, under whom I studied, was doing his best to make known in this country the unity of the disease, a fact we now so fully understand. The treatment always, in those days, included alcohol; and for twenty-four years afterwards, during which the disease was frequently under my observation, I do not remember a single case in which alcohol, in one or other form, was not employed. About 1870 it occurred to me that alcohol was not the always useful instrumen

of cure, but that it sometimes did evil, caused stupor, increased fever thirst, and induced or favoured delirium. In 1872, I began to venture to discard it, and, gaining confidence as I proceeded, I have from that time until now treated thirty-four cases without administering either alcohol or any so-called substitute for it, and that without one death. The result, as far as it has gone, is an entire success, but must, of course, be taken with all the reserve Dr. Macphail so faithfully enforces.

Of this, moreover, let there be no comparison with the treatment of other practitioners. Such comparison would be most supercilious, unprofessional, dishonourable. I claim simply that, like Dr. John W. Trader, who, in the *St. Louis Medical and Surgical Journal* for November 1893, has published the record of thirty cases without a death (in none of which alcohol seems to have played any part), I have been unusually fortunate. But, while protesting against any inference for the non-alcoholic treatment drawn from so small a number of cases, I venture to report the fact as indicative that such treatment rests on as good evidence as any alcoholic treatment, and that no attack made on the absence of alcohol as a remedy need disturb the equanimity or lessen confidence in the mind of any physician who dispenses with its use.

In further support of the correctness of this view I have received many gratifying assurances. Dr. C. Knox Bond, for instance, has sent me a paper to be read to-day, in which he shows that out of 380 hospital cases, affording 33 deaths, or 8·6 per cent., 309 were treated without alcohol, and that to those who received alcohol *in extremis* it rendered no apparent benefit.

Commenting on his facts, Dr. Bond specially directs attention to

2 :—"That a small total

number received alcohol at all, viz., 71 in 380; 309 having been treated without alcohol. That of the fatal cases as large a proportion as 18 in 33, or 54 per cent., were treated with alcohol, and that the total percentage mortality of 8·68 for the series of 380 cases, judged by the average of hospital-treated cases in this country, is decidedly low—the highest figure, 12·63, being well within the limits of such average, the mortality for the other two periods, respectively 7·47 and 7·2, being much below average. Finally, that the latter lowest mortality figure coincides with the circumstance that, during the year 1891 to which it refers, the administration of alcohol dropped to its lowest ebb, viz., 9 in a total treated of 122, being 6 amongst 114 recovery cases, and 3 amongst 8 fatal cases.

“With regard to the total of 71 cases to whom alcohol was administered,” Dr. Bond says, “12 were cases in which wine was given during convalescence only; in 11 other cases wine was given in amount from 2 ozs. to 6 ozs., for various routine reasons. No effect was noted from its administration in these 23 cases, which need not be further referred to.

“Omitting these 23 cases, only 48 remain for consideration, or 12 per cent. of the total number treated; 18 amongst fatal cases; 30 amongst recovery cases. To deal first with the 30 cases which were treated with alcohol and recovered, it may be said generally that they were, as a rule, cases noted as ‘moderately severe,’ ‘severe,’ or ‘very severe,’ the ‘typhoid state’ having become developed in a considerable number of them. Pneumonia, 4 cases; relapse, 10 cases; hyperpyrexia, 1 case; endocarditis, 1 case; intestinal hæmorrhage, case; diarrhœa, delirium, and abscesses being noted amongst other complications.

"Careful analysis of the notes of these cases," he adds, "fails to reveal any benefit directly attributable to the action of alcohol (chiefly in the form of brandy, 2 ozs. to 6 ozs. in 24 hours), in any of these various phases of the disease. Stress is laid by advocates of this plan of treatment on the pulse as an index of effect in alcoholic stimulation in fever. Fourteen cases in which this point was specially noted, showed no slowing or improvement in tone of the pulse. I have always found that the action of alcohol, whether in health or disease, is to accelerate the pulse rate; slowing, if it came at all, late, induced only through fatigue of the over-driven cardiac muscle, subsequent to, and consequent on, its stimulation, a condition to be avoided in typhoid fever. On the other hand, there is noted the occurrence of various complications subsequent to, or simultaneously with, the alcohol administration, viz., diarrhœa, relapse, pneumonia, and abscess—to name the more striking. Nothing at all indicative of the control of the temperature range in the direction of antipyrexia was ever noted, and that such control is ever obtainable in typhoid fever in doses short of the directly poisonous, is probably a popular delusion; nor was duration of fever ever shortened either in primary disease or in relapse. Notable was one case in which 'direct ill effect' of the remedy was noted, to be abated by its discontinuance and adoption of other measures."

Remarkable also is a report, in the *American Medical Temperance Quarterly*, by Dr. Kellogg, of Battle Creek, in which it is shown that in a total of 285 cases of enteric fever treated absolutely without alcohol there were 6 deaths, or 2.1 per cent. These cases, it is true, were all favourably placed when compared with the complicated and semi-moribund cases that are brought

into a London Hospital in any stage of the disease; they are, however, invaluable, as showing that high mortality returns in hospital do not depend upon non-alcoholic treatment of the disease, but upon other and surrounding circumstances and constitutional conditions of the patient, over which, probably, every remedy, even if it were specific in its nature in the early stage, would fail to save life in all cases.

I should like also to point out—it is indeed a duty to point out—that sometimes *in extremis* prescribers are apt to be deceived regarding the efficacy of what may be considered the precise medicinal use of alcohol, and to give alcohol credit it does not possess. This once occurred to me in the most striking manner. I was summoned to meet an excellent practitioner in a case of enteric fever far advanced, and critical to an extreme degree. The parents of the patient were abstainers, and opposed the urgent wishes of their medical attendant that alcohol should be administered. At last a compromise was agreed upon—namely, that I should be called in, and that my decision should be final. I found a young lady, as it seemed, near to death. Her extremities were cold, her mind wandering, her pulse almost imperceptible, her tongue barked. If there was ever a case for alcohol this was one, and my old predilections—shall I say prejudices?—in favour of alcohol, coupled with the arguments and truly conscientious urgings of my medical brother, led me to concede to the extent of prescribing alcohol of sp. gr. .830, properly diluted, in half-ounce doses every three hours. On the following morning early, when we again met, each of us anxious as to what the events of the night had been, we found the scene most agreeably changed. The patient was calm, warm, sensil

I had almost said cheerful. Truly we thought alcohol had scored this time. The supposition was soon rectified. When our prescription, written rather late at night, reached the chemist who was to dispense it, he had in store no alcohol of the specific gravity indicated, and, wishing to be very exact, he sent out for the precise thing. This caused loss of time, and the remedy did not reach its destination until after midnight. By this hour the patient had commenced to rally. She had partaken rather freely of a prepared milk food which had been ordered, and, although there had been some recurrent diarrhoea, she was in every particular so much revived that the parents kept back the administration of the alcohol until our visit, and there it lay on the drawers by the bed unopened. It was never used at all, and it never seemed afterwards to be called for. If it had been used, what a false triumph it would have created for those who call alcohol a sheet-anchor in such instances; and as in their hands the good results might have followed the prescription, how could they be convinced of error? It requires such a fortunate accident as the above to reach them at all, or give them the moral courage to look at both sides of the shield, and make them just, as well as truthful, in observation. No patient could have made a better recovery than the one here referred to.

In the treatment of enteric fever we are not as yet looking at a disease curable by any direct remedy, but on a series of changes in the body, a series of processes which have to be watched day by day until the time comes when a radical or abortive remedy shall be discovered. In this disease the animal heat is so raised that it were no incorrect figure of speech to say that
 a, the body of the patient, is a-fire,

and that the danger is in proportion to the damage done to the several internal organs or parts that are affected by the fevered or increased combustion, and to the wasting of body which is invariably present during the febrile state. It has been calculated that not more than 24 per cent. die from the direct or immediate effects of the enteric poison. The deaths in the majority are due to complications occurring after the first week, 50 per cent. being in the third and fourth week. We have, therefore, to be ever on the watch tower, and that treatment is most successful that is ready, at a moment's notice, to meet emergency.

SUMMARY.

To sum up my task, what I have endeavoured to show may be put under six heads.

1. That the average hospital mortality from enteric fever is from 15 to 16 per cent.

2. That cases of enteric fever, taken in the earliest stages and judiciously treated, may exceptionally yield as low a mortality as even 2 to 3 per cent.

3. That the dangers occurring during the disease are due to complications, and especially to those involving the alimentary canal, dangers much intensified by movements of the body and errors of diet under such complications.

4. That in ordinary hospital practice, into which is admitted every kind of case in every stage, 9 per cent. is the most favourable average.

5. That in spite of complications, so often present when the patient is admitted, it may be possible to keep down mortality to an average of 9 or 10 per cent without any use of alcohol; and that if the Bo

Temperance Hospital is able to supply still greater improvements in baths and other appliances, results still more encouraging may be obtained without any departure from the system pursued in the practice of the hospital, in so far as alcohol is concerned.

6. That there is not the shadow of a proof, based on sound and sufficient statistical records, that abstinence from alcoholic treatment increases the mortality of enteric fevers.

7. That, owing to the small number of cases in which the non-alcoholic treatment has been employed, the results, quite sufficient to prove its equality, are as yet insufficient to prove its superiority as a practice.



PRETENDERS TO PHYSIC.

THE recent trials of persons assuming medical functions without qualification have led me to recast an essay, many years old, bearing on this subject and probably long forgotten.

So intimate and delicate are the relationships between the profession of medicine and the public, that whatever is for the real benefit of the profession is equally for the benefit of the public. The higher the professional standard of medicine is raised, the higher are the members of the great body of medicine placed above want, and therefore above the temptation to do mean acts. The higher the members of the profession are advanced in the social scale, and in worthy public estimation, the better are the true interests of the people, in all that concerns the care of their health, considered, served, and satisfied.

The reason why these positions are as I have stated is plain enough, and is, in the nature of things, positive. That which the sick man requires, first of all, in his sickness, is protection. He is disabled, and must be protected—protected against himself more than against any one else; for the strongest of men and women, the strongest of mind sometimes even more readily than the weakest, are soon off their mental balance when the travails of disease overtake them. It has been in

the range of my own experience, more than once, that the most educated of physicians, and in their periods of sound health the most fervent in their detestation and denunciation of quacks and quackery, have, during diseased states of body—not of a fatal character—been ready to run after the veriest scamps in the ranks of quackery, and to resort to the most wanton and foolish of quackish devices, paying the quacks withal for their knavish tricks, whilst willing and unselfish brethren have been taxed to wait and attend without so much as a thought of compensation for time, anxiety, and trouble.

With such facts as these staring us in the face, what shall we say to the members of the general public if they, in like manner, under the mental alienations of disease, or under alienations of fear and love for those dearest to them, run from the learned and honest to the illiterate and knavish, and seek in the depths of superstition what they think they have failed to find on the heights of science? What shall we do to meet the danger to which these seekers expose themselves, and to lessen the communicating and communicated evils which self-seeking tends to keep alive?

THE FAMILY OF QUACKS.

Quacks are born, not made. Quacks are born criminals, who, by accident of position, are led to practise their criminalities on the issues of life and death. They exist within the ranks of the profession of medicine as well as out of it. A little change of circumstance in the way in which his life was first cast, and the quack had been a burglar, a bandit, a forger, a thief, a receiver of stolen goods, a common murderer. Nay, the worst of these one

of the worst ; for some of these may show qualities of skill and of courage in the commission of their offences, while the quack practises like a mean coward on the innocent and the enfeebled, so that he is amongst the most contemptible as well as the lowest of the criminal community.

The following are the prominent representatives of quackery that commonly come under general notice :—

TERRORIST QUACKERIES.

There is a form of quackery which is essentially of the commonest, if not the wickedest, in the whole field of quackery, which, in the most systematic manner, deals with the more secret and objectionable of human infirmities—I mean, with diseases springing out of sensual indulgence. The men who carry out this line of bad business are numerous, as a class numbering, I believe, in England and Wales alone, from two to three hundred. They are not, as a rule, men who have received any medical education. A few of them may have attended medical classes, and a few may have passed through their curriculum and failed in examination. The majority are mere ignorants, who sometimes assume a medical qualification of a doubtful medical college of another part of the world. They base their claim to be trusted on the ground of the secrecy of their knowledge and the secrecy of their proceedings. The very fact that they themselves are obscure and mysterious is the reason too often why they are consulted by those who, in the matter of their illness, themselves wish to be obscure and under concealment. Many a youth, who, under ordinary circumstances, would go to the family physician or surgeon for advice and assistance,

goes to one of these men because he thinks he can open his mind to such a man freely, and communicate his distress without fear of discovery.

A number of these quacks catch their victims by means of books which they write, and which they advertise largely. In the books so written there is nothing new. They are nearly all founded on one rather old book, which was published in English in the early part of this century, which, from its title, assumes to be the work of one of the ancient Greek philosophers, Aristotle, and which still floats about as a kind of suppressed book, to be read only in secret, and never acknowledged. There can be no doubt that a few of the men engaged in this line of practice acquire money, and some of them are able, under an assumed name, to conceal their abominable avocation even from their own families. They pass themselves off as "City men," or as "merchants," or under some other general term. One of these men once took a sudden alarm that he was about to receive condign exposure, if not condign punishment, from a medical association called the Medical Defence, of which I was then president, and he thereupon wrote to us, imploring us to let him escape. He said that none of his family knew his mode of life, and that for them to know it would be ruin to their prospects. He faithfully promised to retire from the quack business at once, and, in proof of his abhorrence of it, he tendered the gift of five guineas to the Association, to enable it to prosecute another quack of a similar stamp and character! We gathered from this, that these quacks do not entertain the Christian virtue of loving one another,—and accepted the evidence as a hopeful sign.

The greater number

I miserable

lives. If a few of them get wealth, they are afraid to enjoy it ; most of them do not get money, but sink from one lowness to another, until they become the dregs of society. They do an incalculable amount of evil. They not only directly rob, they corrupt their victims. Seizing upon minor failings of a physical or moral kind, they distort or caricature such failings, until their victim is literally distraught with the idea of the life-long misery that is in store for him. Moreover, having raised this misery, they do no mortal thing to allay it. It is a part of their shameless policy to keep the mental wound open, to offer temporary relief at the most, and to maintain silence at the price of a blackmail, which knows no end so long as the fears of the victim hold ascendancy over the acts of his life.

Occasionally, the quack of this school is in the hands of other men less scrupulous even than himself—men who hold him, direct him, use him. In one instance which I have known, the quack, or ostensible practitioner, was a duly qualified and—strange to say—in his better days, a well-informed and genial man. He carried on his nefarious work in a house belonging to his employer, a wretched money-lender, who, acting as servant to his tool, actually kept the door, together with the most rigorous account of every patient who came, and of every penny that was extorted.

The respectable public cannot think we do wrong in keeping an eye on quacks of this order. If the public understood its own dearest interests ; if it understood the interests of those youths who spring from it, and who are to become the backbone of the next generation, it would give more than thanks to any vigilance ; it would give its earnest assistance—

“ To whip these scoundrels naked through the world.”

"Resist the devil, and he will fly from you," was never better illustrated than in their histories. The merest attack on them disturbs their success; and, in short, they exist simply because they are permitted. The public gives them a tacit licence, and that is their diploma. Supported by the public voice, a good organisation could put them all down in twelve months, and relieve the young of their pestilent influence. Happily they are on the decline.

HOMICIDAL QUACKERIES.

There is another entirely different order of quackery, more dangerous, and, in one sense, more directly mischievous, because it is even murderous in intent. The quack of this school has his accomplices in the female sex, and works in combination with them. He carries out one of three courses, or sometimes two of three; or, again, three of three. These courses are—(1) to stop life, at its beginning, by artificial means; (2) to produce abortive birth; (3) to destroy the life that has been produced in the first stage of its existence.

The class of criminals who are thus employed are far more numerous than many suppose. The rewards for their crimes are great; and it sometimes happens that the persons who employ them hold what is called "position" in society. The crimes of the bulk of these are very difficult to trace home, as the records of the coroners' courts on the still-born too convincingly prove. With some of these men a certain amount of skill, acquired by actual practice, enables them to destroy the infant life and save the life of the mother so adroitly that conviction becomes almost impossible. It is only when the unfortunate mother has been deceived by her

offspring that any discovery of a true and convincing kind is made.

Offenders of the second and third orders of this family are too often shielded by a legalised diploma, and evade discovery by a perverted knowledge and skill.

NOSTRUM-MONGERING QUACKERIES.

There is a third order of quacks, the members of which are not perhaps intentionally criminal, but who, nevertheless, take upon themselves duties and functions that are altogether foreign to their knowledge and skill. It is not so much that they practise irregularly, or without licence, as that they pretend to treat diseases with the names of which they are not correctly familiar, and with the nature of which they have no conversance whatsoever. Some of these quacks take one particular disease or one particular set of organs of the body under their pretended care; others assume to treat diseases in general, in which case they commonly pretend to possess a particular kind of remedy which adapts itself to every kind of case. These last-named, owing to the method they pursue, attain, by the rule of chances, a certain measure of success. Vending some one active medicinal substance, such as opium, quinine, or iodide of potassium, and supplying the remedy to a great number of persons indiscriminately, they are sure to give it, every now and then, to a person who will be benefited by it, and so, in time, they get together a list of successful cases, or, as they call them, cures. This was signally the fact about fifty years ago, in the career of a noted quack woman of the midland counties, whose remedy was simply quinine given under various disguises, but always in full and effect.

doses. There are so many forms of disease in which quinine acts as a splendid remedy, it is no wonder that this quack had a long list of "cures," each of which, magnified in number by admiring credulity, gave a glamour to her practice which no regular practitioner of medicine could legitimately attain. The same kind of success is, I believe, at this moment attending "a mode of cure," as it is called, in which the curative agent employed is potassium iodide. Others, again, in this family of quacks, bring out a particular remedy for one particular disease, and try no further. I believe that, without an exception, the men who follow this plan do no more than use the prescription of some well-known physician or surgeon, which prescription has either been published or has come into their hands from the original. One of the most largely sold specific pills was, to my own knowledge, made from a formula in the day-book of a late eminent general practitioner. The vendor of this pill, who assumed a new name in order that he might carry out his design without being generally known, made no secret of the source of his remedy to the person from whose book he originally purloined it. This family of quacks is very large and industrious, and, I fear I must add, successful. The members of it make fortunes, and they connect themselves so closely with the mere sale of their nostrums, without making a pretence to prescribe or practise, that they succeed usually in evading all the existing laws which might be considered as reaching them. It is fortunate for them, moreover, that, although by their haphazard method of dealing out active remedies they often inflict injury, it rarely happens that the injury is fatal.

SPECIALIST QUACKERIES.

There is a large family of quacks, the representatives of which carry out what they are pleased to call a "new old" system, or a distinct system of medicine. Some of these call themselves "botanic practitioners," or "herbalists." These practitioners are very numerous; they exist in every large town. In their practice there is nothing diagnostic, neither have they any scientific art or skill of a therapeutical character. At the same time, they hold in their hands drugs of a most potent kind, which they use boldly, and often with the production of extreme effects on the body. Lobelia is one of their prime agents, digitalis is another; and by the application of such active medicines they occasionally inflict a fatal injury. Then they are brought up before the coroner, and are perhaps committed for trial. In most cases they escape punishment, and proceed as before. Many of these persons are not intentionally doing wrong. They are the last of the old herbalists of the Culpepper school, and of a school of medicine proper which existed prior to the time of Boerhaave. They cling to the crude notions they believe in with all the tenacity of superstitious fervour. From this fervour they make others trust them, and produce a sect of believers in them and their doctrines. They do not mix with other charlatans, neither do they come by accident into their business. "They learn their business," as one of them told me, "in a regular way, like other tradesmen"; and some of them still retain the remnants of astrological traditions. I once met with one of these, who, not knowing me, entered into a long argument with a bitter grievance. He assumed that he and his class were the true desc

old Esculapian stock; that they had a certain remedy for every complaint, learned by experience; and that the "regulars" were a kind of usurping race, who conquered the ancient princes and ruled by force over the ancient and inspired fathers of Physic, Galen included.

Akin to the "botanicals" are the "medical electricians" and the so-called "galvanisers," who are becoming trained in their practice as a vocation. These are often mere ne'er-do-wells, who learn a smattering of electricity, and, obtaining a battery, "apply the current" without regard to one single physiological consideration. Singularly, as electrical learning improves they grow in influence as well as presumption day by day, and by extolling the true discoveries in electricity, and expounding the value of electrical science in the help of mankind, they make it appear that in their sphere, the application of electricity to the cure of disease, they do what the Faculty omits or neglects, and are therefore the pioneers of a new practice and school of physic. This, they say, is their *raison d'être*, so that they are a necessity of the age.

There is still another group of this same family of irregular practitioners, the members of which claim to have a special skill in the treatment of the skeleton. These trade under various names—"bone-setters" being the name most commonly applied to them. They exist in or near many of our great centres of population, and as they, like the botanic specialists, are born to their work, their business very often descends in family line. They have no necessary knowledge of anatomy, and no method which they care to disclose to those who would learn from them. Their common practice is to find a dislocation, and to do something, much or little—and, too frequently, much. I know

an instance in which one of these men, mistaking a strumous suppuration near the shoulder-joint for a dislocation, actually pulled the humerus out of its place, and produced a true dislocation of the head of the humerus into the axilla. These bone-setters are popular, and are defended by popular superstition. Raised to the place of legitimate practitioners, their skill is gone, and their credit with it. The late Dr. Robert Willis told me he knew one of these men, who, after having been bred to his empirical calling, came to London to study anatomy in the regular way, and who was so astounded at a description of the carpus given by Abernethy, that he became faint from the remembrance of the manner in which he had manipulated that exquisite piece of mechanism in his ignorant practice. From that moment he lost all faith in a skill which he had previously conceived was of the most perfect order, and which he had ruthlessly exercised.

The favour of some of the wealthy of the community has encouraged these quacks to multiply; while the semi-patronage which has been given to certain of them by some professional men, as if in their stupid mystery there was really some valuable lost art, has helped not a little the public credulity. I have been informed that certain of these quacks have actually secured the assistance of duly qualified practitioners to administer chloroform or other anæsthetics for them during what they have had the audacity to call "their operations"! The last of the most notorious of this fraternity in London died some time ago, and with him much of the imposture he represented.

Connected somewhat with the bone-setting empirics may be mentioned another mechanical *fratern* modern growth, which employs itself in *iro*

The unqualified practisers of this craft pretend to a knowledge of deformities of the skeleton, and especially of the spine. They assume to treat diseases in the most pretentiously systematic manner, speak openly of their "patients" and of their operations, and get large fees for locking up poor human beings in iron cages which would have done no disgrace to the Spanish Inquisition. They have inflicted endless mischief, too often on the feeblest members of the enfeebled community. Happily, they also are wearing out as a successful fraternity.

PROTECTED QUACKERIES.

There is a fifth family of quacks, which, assuming the manner and character of general and fully licensed practitioners of medicine, is given to practise without any licence whatever under different shades of protection. These men have caused a deal of trouble. They are sometimes men who have gone through a certain part of a regular medical education; they are sometimes men who have had no education at all; and, again, they are sometimes men who follow at the time they are engaged in their quackery certain other occupations in which they have been trained. They fill up their leisure in the quack practice. The immunity which these different kind of quacks experience is due to the protection they get from traitors in the camp of medicine itself. They put up their names as curers side by side with the name of some duly qualified medical man. The traitor signs the certificates of death in cases of death, rushes in in emergencies, and, in a word, covers his friend with the protection of a
which he has sworn shall never be degraded.
disclosure to have to make, but unfor-

unately it is as true as pitiable. There have been instances where the protecting man has been residing nine and ten miles away from the protected. A mere sense of greed, rarely of necessity, leads to the course of conduct so subversive of all professional morality, truth, and honour.

This family of quacks which may be called *the protected family* is on the decline.

COUNTER PRESCRIBING.

Those who are charged with this irregularity are dispensers of medicine—druggists and pharmacutists. The extent to which their form of irregular practice is carried out is extremely variable. The highest class of pharmacutists discard it altogether; the lowest class cultivate it sedulously, and, no doubt, turn it to a profitable account, trespassing largely on the province of medicine proper. The majority carry out the practice in a modified form, giving as little advice as possible, yet never refusing it altogether—believing, in fact, that it is a necessary part of their vocation. According to the strict letter of the law, the gentlemen who carry on the counter-prescribing business are constantly breaking the law, but no one would ever dream of classifying them with the other families of the baser sort which have been described in previous sentences. The work of the Pharmaceutical Society has tended to make pharmacy a science, with which medicine is now in friendly and recognisable communion, to the ultimate advantage of both and of the commonwealth.

PERIPATETIC QUACKERY.

Under their different heads the various classes of irregular practisers have now been placed. There is a

remnant of another class which once was the first of all—I mean the old peripatetic, who wandered about to sound of fife and drum, a truly historical being, and now and then a veritable philosopher. But the remnant is a remnant now. In some English cities there remains still a licensed corner of the street in which the peripatetic may, at stated times, stand and “cure”; and there are also several market towns in which he still keeps his stall, sells his vermifuges, and exhibits his pathological triumphs. Taking him at his best, he is, in this day, reduced to worms and warts, with an occasional invasion into corns and bunions. He is almost too harmless to be cared about. *Sic transit!*

I have felt it would be worth the time thus briefly to classify the different orders of irregular practice. There is nothing at all that is new in these descriptions in the way of information to the profession or even to the public; but I have tried with some pains so to marshal the facts that we may see more clearly the work of reformation that lies before both the profession and the public. To that topic let me next ask attention.

THE SUPPRESSION OF QUACKDOM.

The fact at once stands out, that the evils to be met in the suppression of quackery are very complicate, and are only to be dealt with by the process of treating them one by one, and seeing how they can finally be grouped together to be brought under practical legislation. I use the word “legislation” with a sigh. It is necessity, not desire, which enforces its utterance. I know it would be far better and far happier for us who practise medicine within its legitimate boundaries if we

had no occasion to ask for legal protection, and if people would consult legalised practitioners only. But, alas ! there is so much crime, both in the way of omission and commission, amongst the irregulars in medical practice, it were itself a further crime to wait for the day when general enlightenment shall render the rule of law the rule of oppression on the one side, and of privilege on the other. We must still have some legal protection for the afflicted, even from themselves, from their weaknesses and vacillations, and for their own interests. We must have it specially for those who, being young and innocent, are not as yet a law unto themselves, but are under the direction and guidance of their masters and directors.

It is probable, I dare say, that the day will come when all the learned professions will cease to exist. We none of us can fail to discern, if we use our discernment, that almost all the evils we have to contend with are due to the follies or ignorances of the human world. In law, an insanity of purpose in his *clientèle* keeps the lawyer employed. It is next to impossible to suppose a lawsuit in which one of the parties concerned is not, to some extent, insane, while in many instances both parties are wildly insane. In medicine it is most difficult to find a case in which some error of judgment, or action of the present or of past generations, is not the root of the evil; an error which a better and sounder knowledge and foresight will ultimately prevent. But while these varied insanities and errors last, some persons must be duly trained to study and meet them for the time, and by the study and practice to suggest the final means for their complete removal. Such persons must, in their turn, be duly protected in their high vocation ; while those who do not dare to face the same

ordeal of training, and who, from ignorance or selfishness, claim to rank with the qualified, must be put down by all fair and honest means.

When we look back upon the different qualities of the different classes of men who make up the irregular corps of physic, we soon discover that different methods of treatment require to be pursued in the process of suppression.

The groups of pretenders included under the heads of the terrorists, the nostrum-mongers, and the specialists, would best be dealt with by a new Medical Act, which shall not merely prevent these men from assuming false medical titles, but also suppress them at once from assuming false medical practice.

Over the heads of the terrorists there is some additional authority. In order to carry on their loathsome craft, they constantly come under the Act for the suppression of obscene publications and practices, and by a vigilant observation of their *lâches* in this respect they could be brought under instant subjection. To some extent, they are now suppressed; but not efficiently.

It has been suggested that protest against the patent medicine system should be made with a view to urging on the Government its speedy abolition. But the fact is that the advertised quack remedies that are patented are generally innocent trash when compared with the unpatented preparations. I rather think that the patent laws might be enlarged, to the benefit at once of the public health and of the revenue, by enforcing that no medicine should be advertised as a specific unless it be not only patented, but the secret of it be disclosed. I also think that great good would come from pressing the rules of the Sales of Poisons Act on the sale of many poisonous preparations vended as medic

The two classes of men whom I have called quack homicides and protected quacks are not so easy to suppress. They sail, as a general rule, directly or indirectly, under the legal diploma, and they use the very means which are intended to protect the public interests for the purpose of inflicting injury and even death.

In reaching these persons, recourse so far has been to watch the proceedings in the coroners' courts in cases where good ground for suspicions has been obtainable, and to insist that the inquiry shall be carried out to its fullest fair degree.

My own view is that an amended system of registration of the still-born would do more to suppress secret crimes, committed by the classes of pretenders now under consideration, than anything else; more even than the establishment of a public prosecutor, valuable in executive action as such an officer would be. I further incline to an idea originally suggested by the late Sir Edwin Chadwick, that the medical officer of health ought to be an officer formally entrusted with the corroboration of all certificates of death; that every such certificate should pass under his supervision, and should not be officially received as a permit for burial or other method for disposing of the dead until his consenting signature appears upon it.

The class of irregular practitioners who cause most anxiety, from the wish to be scrupulously just to them, is the class called the counter-prescribing class. Without for a moment admitting that in law there is any class privileged to prescribe out of the ranks of medicine, every one must feel that the dispensing chemist is in a most difficult position, and that he is sometimes even obliged to break the letter of the law.

It is inevitable that so soon as a man begins publicly

to sell medicines and remedies, buyers will begin to seek advice as to remedies from him. It is inevitable also that in emergencies he will be called, by the purest dictates of humanity, to supply remedies according to his best skill and judgment. And I, for one, hold that the time has come when the difficulty of counter-prescribing should be met, not by levelling down, but by levelling up; that is to say, by making such prescribing a legal practice in the hands of a specially elevated and examined class of practical pharmacutists. In the examination which should be provided for the purpose now proposed, there is no necessity to think of anything commonplace or superficial. The pharmaceutical examination which at this time exists, and which has already half anticipated the movement, might stand as it now does at the Pharmaceutical Society, and, with the exception of midwifery, all those parts of the present examination for medical licence that are not strictly included in the pharmaceutical division might also stand as they are. For this combined examination there would soon be plenty of excellent candidates, if the admission were reasonably accessible.

The qualification thus granted, and extending solely and legitimately to counter practice, never to out-door visiting, would at once smooth away the present difficulties and persistent annoyances. It would define responsibilities and duties in the clearest manner; it would not give the dispenser the right to sign a certificate of death, nor any medical certificate whatever; it would not give him the right to call himself a registered practitioner of medicine; but it ought to give him the right to dispense publicly medicinal substances with qualified judgment.

With the class of men who would by these steps become the pharmacutists of this country, the professors of medicine and surgery proper need have no cause for quarrel nor jealousy. They would be highly educated men, occupying a true scientific position; and they would be such able dispensers and professional allies that all who were engaged in practice purely would cease to feel any necessity for dispensing medicines, or for doing anything more than attend the sick and prescribe for them.

The suggestion made many years ago is based on the principle that the public necessity, which is the first thing to be considered, must be considered apart from professional prejudice, apart from what merely seems good for ourselves, and apart from blindness to the open fact that what is best for everybody is best for our profession and for every profession.

There is yet one other means for the suppression of quackdom, and that lies within our own borders. I am not about to refer to men who, duly qualified to practise the healing art, degrade their calling by carrying out methods which are intended, like those of the barefaced pretenders, to defraud, nor more nor less. I would notice rather, with regretful criticism, the spirit that encourages the trade rather than the art; the spirit, every day becoming more explicit in its manifestations, that plants what is called success in medicine, on success in obtaining, through medicine, piles of wealth, and in leaving at death the vast fortunes which the debased selfish world alone envies. Once it was the honest pride of medicine to look for all honour in work leading to the advancement of the science and the art. Once, when a man of mark died, he died a man of mark because he was a man of work · and his

cotemporaries said of him, "This is what he has done ; and his works do follow him." Now it is never inquired, "What is the value of the man in his splendid vocation as a labourer there?" but, "How much money per annum does he realise? How much has he left behind him?" Has he left his thousands in three figures, then he is great ; in two figures, then he is commonplace ; in one figure, then he is too small to be regarded—let him have the contempt he has earned, the bare silence of the grave.

I say not a word against a worker in medicine making a competency. Every one should make that, if it be but to support Rochester's wise suggestion that it is good to be competent in order to be honest. It is against the process of weighing out talent by guineas that protest should be made. No height of qualification, no force of law, will suppress rampant quackery until this internal reform of pride in worth *versus* wealth is the keystone of the royal arch of our noble craft.

OPUSCULA PRACTICA.

"There are mites in science as well as in charity."

BENJAMIN RUSH.

A NEW ESOPHAGEAL STETHOSCOPE.

DR. SOLIS-COHEN, of Philadelphia, has been good enough to send me an account of a new kind of esophageal stethoscope which he has invented for intra-thoracic auscultation, after the manner described in ASCLEPIAD, vol. ix., p. 371. He says that in attempting to repeat my observations he found that there was occasional danger of the regurgitation of fluid through the tube into the ear-pieces. To obviate this objection he has introduced between the stomach-tube and the ear-tube a rubber capsule containing a diaphragm of gold-beater's skin, which serves as a barrier and as a resonator. The capsule can be readily taken apart and cleansed, and new diaphragms can be inserted when needed.

His esophageal tube is of soft rubber, No. 27 of the French scale, having a lateral eye near the blunt tip. On the same side as the eye, beginning five inches therefrom and extending up to a distance of nineteen inches, is a series of indentations, one inch apart, which show how far the eye of the instrument is from the patient's teeth. By experiments made outside of the body he has found that the sound heard is transmitted from within a radius of an inch and a half or two inches

from the opening in the tube, the sound being most distinct when the eye of the tube is in close apposition to the source of sound. He has used the instrument to listen to the sounds of the heart only, not wishing to risk the passage of the tube in cases of thoracic aneurism, and not being able to hear through it the lung-sounds.

The most striking difference, says Dr. Solis-Cohen, between the sounds of the heart as heard in the ordinary way and the same sounds as heard through this instrument is the almost uniform quality of the first and second sounds—that is, the sharpness of the second sound, as heard by the ordinary method, is wanting in auscultation by this method. The rhythm is of course the same; the second sound is the shorter, and the pause is easily appreciated. It is possible to separate the two sounds and hear the first sound alone, or the second sound alone, or to hear both. At a distance of from eight to nine inches from the teeth, only the second sound is heard; at ten or eleven inches, both sounds are clearly heard; and at thirteen or fourteen inches only the first sound, with perhaps an ill-defined second sound. He has not satisfied himself as to his ability to differentiate between the aortic and pulmonary sounds by varying the position of the eye of the tube.

His largest number of observations have been made on persons with healthy hearts, who were under treatment for stomach troubles, and thus accustomed to passage of the esophageal tube; but he has been able to use the instrument on other patients, with and without cardiac disease; among them two cases in which by the ordinary method of examination he had not detected the existence of mitral obstruc-

tion. In one of these two cases, mitral obstruction alone was diagnosticated by the ordinary methods of physical examination; and in the other, mitral obstruction and regurgitation. In both cases he failed in each of two examinations to hear the obstructive murmur through the esophageal stethoscope. The regurgitant murmur was heard distinctly, but he could not make out that any part of it was pre-systolic. He does not know whether there is an acoustic reason for this, or whether it was due to lack of skill with the instrument. He has no doubt of the correctness of the diagnosis.

He has also used the method in one case in which there was doubt as to the coexistence of aortic diastolic and aortic systolic murmurs, but the apparent murmur of regurgitation was not heard through the esophageal stethoscope, whilst the systolic murmur was heard distinctly, and a good closure sound was heard. This, he thinks, warranted affirmation of arterial roughening rather than valvular lesion.

Of the importance of intra-thoracic auscultation, he cannot as yet speak positively. He does not know how far it will be applicable in cases of doubtful diagnosis—in others it is not needed—or what its exact limitations are. He considers it, however, one of the methods that we may usefully employ among others in making exact observations, and for this reason brought the subject before the College of Physicians of Philadelphia for discussion.

These observations of Dr. Solis-Cohen are very confirmatory, not only of what I have already written, but of researches since conducted.

Why the sounds of the heart are heard so differently and in so subdued a manner from within the thorax, as

compared with the sounds outwardly heard, is, I think, due to the fact that the resonance of the chest wall aids us in the external use of the stethoscope. I have not yet had a case of aneurism in the thorax in which I have failed to obtain a local murmur from within the thorax; but it is rather a whisper than a murmur, I presume from the same cause.

In a patient who was under the care of my colleague, Dr. Ridge, for some stomachic affection, and who had himself learnt to pass the esophageal tube so frequently that he had used it two hundred and forty times, I got a good subject for intra-thoracic auscultation. The patient could move the tube to any point I wished without inconvenience to himself, and I was able at leisure to listen to the cardiac sounds, and to explore stethoscopically from the mouth down to the stomach inch by inch. Some difficulty arose from fluid filling the tube, which Dr. Solis-Cohen's capsules would probably have remedied.

This, one of the most careful examinations I have made, confirms my previous account in every detail. The fermentative bruit in the stomach was well marked; the first and second sounds of the heart were distinct; but the vesicular murmur of the lungs was, as before, deficient, so that indeed no breath sound was detectable until the open end or eye of the tube reached the level of the larger bronchi; then, bronchial sounds were very loud, and the voice sounds singularly sharp and strong. The one objection to the frequent employment of the tube is the retching it produces, an objection which will often stand in its way.

OZONE AND INFLUENZA.

DR. HENRY B. BAKER, Secretary of the State Board of Health, Michigan, in an admirable report issued each year bearing on the sanitation of the state, devotes a special section to various meteorological conditions in their relations to prevailing diseases. He divides diseases into two classes, those of cold, and those of warm weather, with particular reference to the amount of ozone in each division; and he lays down as a proposition that in months when *more* than the average per cent. of weekly reports stated presence of pneumonia, or of membranous croup, diphtheria, tonsillitis, influenza, scarlet fever, rheumatism, neuralgia, pleuritis, or pulmonary consumption, the average daily range of temperature, the relative humidity of the atmosphere, the average per cent. of cloudiness, the ozone, the average velocity of the wind, the monthly and the average daily range of the barometer, and the average daily pressure of the atmosphere were *greater* than in the average for the year. But in months when *less* than the average per cent. of the reports stated the presence of the diseases named, the meteorological conditions were less than the average for the year. (Report for 1890, page 129.)

In a later report, to which I see Dr. Andrew Wilson draws attention in his "Science Jottings," the subject of atmospheric ozone and influenza is discussed. The report shows that when ozone is abundant influenza is particularly in evidence. The bacteriologists will, of course, connect this in some way with their favourite hypothesis; but what I want to point out is the fact that there is no necessity for any argument of the kind they may adduce. Readers of ASCLEPIAD have only to tu

to vol. viii. (for 1891, p. 180-2) to find that ozone itself is a sufficient cause for the production of the symptoms of influenza, and that the symptoms induced by ozone constitute a perfect synthesis of the disease, and of the pneumo-paresis.

*CHLOROFORM ADMINISTRATION IN SMALL,
REGULATED DOSES.*

TWO tables of administration have been submitted to me in which numbers of persons have been successfully subjected to anæsthesia by means of the regulating inhaler referred to in a previous number.

The first table includes twenty-five cases in which Dr. R. W. Carter was the administrator; the second, a list of one hundred operations performed by Professor Vincent, Surgeon-in-Chief to the Hospice de la Charité, Lyons, in which the chloroform was administered by the regulation method. The results are extremely satisfactory. In Dr. Carter's cases there was an average of administration of 5.16 minims of chloroform—5.93 cubic inches per minute—with induction of anæsthesia in continuation for periods varying from 15 to 155 minutes. In Professor Vincent's cases the averages were practically the same. It is shown that the time required for anæsthesia by this method is much increased as compared with the rapid administration so commonly carried out; but time ought to be no element in administration if safety be secured, a fact that will have to be further insisted and reported upon. In children the quantity of chloroform used was greater than in adults—a circumstance explained on the view subject to shorter operations in

greater numbers, and that the longer time patients are kept under the influence of chloroform, the smaller is the percentage of vapour used.

ANÆSTHETIC MIXTURES FOR ADMINISTRATION.

Attempts have been made to modify the effects of chloroform, and to render it safer for administration by mixing it with other fluids. Chloric ether (a mixture of chloroform in alcohol) was used, at the suggestion of the late Jacob Bell, before chloroform was introduced independently as an anæsthetic ; and so soon as chloroform came into common use it was tried in various forms of admixture. Ether and chloroform ; alcohol and chloroform ; alcohol, ether, and chloroform have been combined and applied with varying success. A mixture, composed of one part of chloroform and six of absolute ether, was once the most extensively used of all these compounds, and I believe it to be one of the best. At the request of Mr. Spencer Wells, I once administered this mixture while he removed the upper jaw from an adult person, and again while he performed the operation of ovariectomy. I have also used it many times for minor operations, and compared it with other mixtures over which it seemed to me to have advantages. But, after all, I could see no great advancement in this or other mixtures over the simple agent. Snow was early to point out that, when we employ a mixture of ether and chloroform, we produce narcotism with the lighter ether and keep it up with the chloroform, so that there is a mixed effect, and this is essentially true of all mixtures that have been constructed. Let us carefully look at the physical properties of three agents—chloroform, ethylic ether, and ethylic alcohol—and we shall see how different

they are, and how improbable, I had almost said impossible, it is that they should act together; that is to say, that they should at the same temperature evaporate uniformly and equally, diffuse uniformly and equally in the air, and condense uniformly and equally in the blood.

	Specific gravity. Water = 1000.	Vapour density. Air = 1.	Boiling point.
Chloroform . . .	1.491	4.141	142° Fahr.
Ethylic ether . . .	0.720	2.586	96° „
Ethylic alcohol . . .	0.793	1.613	173° „

We cannot look for a moment at these simple facts without seeing how puerile it is to expect uniformity of result from the evaporation and diffusion at the same temperature of bodies so physically different and only mechanically admixed. In evaporation the three agents split up into their three distinct vapours, each expanding its own natural way, diffusing its own way, condensing its own way, and, what is more, each playing its own natural part in physiological action. To sum up, when we mix chloroform with another agent, we weaken it as if we diluted it in air to reduce its effect, so that the question of mixtures brings us back to the previous question of quick or slow narcotism, which has already been discussed.

If it were possible to get two fluids which were near to each other in physical characters, we might in a mixture of them obtain a compound on which to base a scientific research on the effects of admixture. We might take bichloride of methylene and ethylic ether for this purpose, as will be seen when these two bodies are compared.

	Specific gravity. Water = 1000.	Vapour density. Air = 1.	Boiling point.
Bichloride of methylene . . .	1.344	2.937	104° Fahr.
Ethylic ether . . .	0.720	2.586	96° „

And, I could offer other combinations which are nearer than these in relation to physical constitution. Now such analogous bodies, I admit, deserve to be studied in action alone and in combination, for one of them might be discovered to produce physiological effects corrective of dangerous faults in the other, and a grand result might be obtained. But the empirical method of mixing fluids of different qualities for the assumed purpose of obtaining uniformity and safety of action partakes of the philosophy of the hospital patient who partook in one dose of a small quantity of each fellow-patient's medicine. As a serious proposition it indicates a scientific coupling, in which love for something new dallies hopelessly with idleness in research.

CHRONIC DIGESTIVE FAILURE FROM SEA-SICKNESS.

THE effects of sea-sickness usually pass away so soon as the victims of it regain the land and have secured a few hours of rest. This, however, is not the invariable rule. In some persons, mischief is done to the organs of the circulation from the repeated mechanical strain produced by the retching and vomiting. I have known one very striking example of this kind in the case of an eminent member of our own profession, who died from an aortic aneurism which had its origin in sea sickness. But the most frequent mischief I have known as a result of sea-sickness is a persistent and troublesome dyspepsia. On the day when this opusculum is written a patient has been before me, one of some ten others who have been before me at different periods, presenting the symptoms, to which reference is to be made, in a

marked and characteristic degree. The symptoms in all their history are typical. The sufferer is usually of middle age, and has not, as a rule, been liable to indigestion, but has always been "a bad sailor," easily, that is to say, made sea-sick. The patient I have just seen was of this class; he feared at all times to cross the Channel, but had usually after a voyage, in which he invariably suffered more or less, recovered immediately, and sometimes even thought himself better from the attack. This time he had been on the sea nine days, and had never been free altogether of sickness the whole time. On landing the sickness ceased to occur in the active form, but nausea remained, and a dislike for food. Several days passed before there was any relish of food of any kind, and when desire returned it was quickly satiated. All kinds of fatty foods were nauseous, sweet food caused acidity and water brash, a light meal partaken of at night led to disturbed sleep and nightmare, any trespass over the lightest meal at any time induced nausea, and a rather heavy meal led to actual vomiting, which brought temporary relief. The matters vomited were partially digested food, with acid fluid; and, no other symptoms, apart from temporary depression and faintness when "the sickness was on," gave any distress. The tongue was clean, the bowels regular, the urine natural, the circulation sound. In a word, there was no symptom except what was referable to the stomach, and the stomachic disturbance was always developed after food.

A curious fact stated by this last patient, as well as by one, at least, of the other sufferers, was that when the nausea was present there was the same sensation of motion, from the rocking of the vessel, as was felt when on board the vessel. If this were not

a fancy, it were a strange phenomenon, reversing that which we commonly accept as to cause. We are led to suppose that the cause of sea-sickness is the motion of the vessel acting through the sympathetic upon the stomach. Here it seemed as if the stomachic disturbance produced the sensation of unsteadiness of movement and the rocking of the body that occurs on the vessel. It may be that the sensation is due to a recurrence of memory, a kind of echo of the impression of the original attack.

In the treatment of the dyspepsia under notice, careful regulation of diet and sedative remedies appear to render best service. The diet prescribed is three light meals of mixed food per day, with avoidance of sweets, tea, and effervescing drinks. I also forbid smoking. Medicinally nitro-hydrochloric acid, with tincture of nux-vomica and an occasional alterative, is ordered. Under this course the symptoms are usually relieved, but a slight excess or indulgence often gives rise to the indication of relapse, as though the nervous governance of the digestive function were permanently reduced in tone.



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Marcellus Malpighius

*Autotype from an engraving
by W. Holl - for J. J. Pettigrew F.R.S.
after a painting by Tabor.*

B.H.R.

MARCELLUS MALPIGHI, M.D., F.R.S.

THE name of Marcellus Malpighi, renowned since the middle part of the seventeenth century, holds a distinguished place in all our medical treatises of the physiological order. His work in medicine, mainly physiological and anatomical, has always passed as true coin of science. Engaged in studies which were in their infancy in his time, his means for research were limited, yet whatever he did bears upon it quite a singular evidence of correctness, extending often to minuteness of detail.

His life in this way becomes, though it be short and uneventful personally, a mark for true scholars and a piece of fine scientific history when it is faithfully presented.

Malpighi was born just one hundred years before the the famous surgeon, John Hunter. His birthplace was Crevalcuore, near Bologna, and the day of his birth was March 10th, 1628. His boyhood seems to have passed without any peculiar predilection for any profession, and he reached man's estate, following general studies in Bologna, giving probably his choice to all that related to natural science. In the year he became of age he lost both his parents; and being left to himself, he made up his mind to let medicine become the pursuit

of his life, with a leaning to that part of it that was philosophical in character and had a bearing on physiological and experimental rather than on practical and clinical researches. He entered as a student of medicine into the university of Bologna, in his time one of the most famous medical schools in the world, and his two first masters were Professor Bartholomew Mascari and Professor Andrew Mariano. To Mascari he seems to have been devotedly attached. He lived at first in his house, where there was a private school of anatomy, and commenced his anatomical investigations there. Later in life he married the sister of Mascari.

I infer that under Mariano he studied medicine proper, but this professor would be a man of advanced age; he had held his chair nearly forty years, and some time before his death, in 1661, he retired into the country to end his days.

Malpighi took his doctor's degree on April 6th, 1653, that is, in the twenty-fifth year of his age. His graduation thesis was devoted to the works of Hippocrates, whose thoughts and labours he endeavoured to revive, a task not very popular at a time when the Hippocratic philosophy was at very low mark, and when the influence of an experimental sect, dealing largely with alchymical problems, had risen out of the labours of what has been called the Arabian school, in which the Semitic element of learning played a most important and distinctive part.

The fervour in favour of Hippocrates and of the strictly practical method he founded was apparently of temporary duration, for Malpighi seems soon to have become distinguished in new studies relating to anatomical, physical, and experimental subjects.

He attained very rapidly—namely, in 1656—as the

gift of the Senate of Bologna, a chair in his own university, but soon afterwards accepted a much more valuable appointment at Pisa, on the invitation of the Grand Duke of Tuscany, namely, Professorship of Physic in that university.

Malpighi, as he has himself told us, was not constitutionally strong; and he found that the climate of Pisa was not suitable to him. He held therefore his new professorship for a very short period—three years—and going back to Bologna, took up his former duties there. After another short interval he was tempted to move to Messina to take the chair of Professor of Physic at the university there, in the place of Doctor Peter Castelli. This post, again, was displeasing to him; and in spite of all solicitations and monetary considerations—for the chair was a very valuable appointment—he once more, after four years' absence, resumed his work in his native city, and remained in it until 1691, when he was summoned to Rome by Pope Innocent XII.,—who, as Cardinal Pignatelli, had known him at Bologna,—to become physician in chief to His Holiness, chamberlain in the Papal Court, and some, but not very reliable, biographers say, a prelate, as though he had taken orders in the Church.

THE LABOURS OF MALPIGHI.

The true scientific career of Marcellus Malpighi commenced at Pisa, where he had the good fortune to become the friend and colleague of the great physicist Borélli.

The life of Borelli will, I hope, yet be noticed independently in these pages; but I may say here that no man could have been more fortunate than Malpighi having such a colleague.

Borelli was at that time in middle life ; he was full of energy, enthusiasm, and invention, and he was at the head of the iatro-mathematical school, which had then sprung into vigorous existence. From its origin this school distinguishes one of the most remarkable epochs in medical science ; it has sometimes been supposed to have originated from the labours of Newton, but it commenced long before the Newtonian era, and was rather a product of the Italian schools than of any other. Borelli was favoured by social position, being a friend of the great house of Medici, and though by mixing in political strifes he had to move about and find shelter at last in the city of Rome, he never lost his scientific leanings or his love for those who followed his own lines of scientific thought and action.

Malpighi, evidently greatly influenced by this master, became a leader in physical sciences bearing on medicine, but not precisely after the same method. Malpighi's work rested on direct observation and what I may call the direct configuration of natural structures rather than on speculations relating to function. Even mathematical rules would not draw him far into speculative controversies. He came ; he saw ; he conquered : that is to say, he threw himself into Nature, he looked her in the face, and he conquered her by marking down what she said, observing her phenomena and simply reasoning upon them, leaving others to interpret. Hence he became a discoverer of parts of the bodies of animals, as other men become geographical discoverers by traversing new regions of the earth and describing them.

Again, for his day, Malpighi became a discoverer after a manner novel and, I doubt not, curious to first followers. He did not, like the previous anatomists, such as Aristotle, Vesalius, and Harvey, content him-

self with considering organs and parts in their mass. The older masters, doubtless, proved useful to him, as having done work which all could learn from, leaving much for their followers to glean. But Malpighi opened a new field: he anatomised parts of viscera, looking into them as his predecessors had looked into the body at large, and finding new worlds, as it were, in the little regions which he investigated. Towards this research he was led by the microscope, an instrument then in its infancy, but with which he really performed wonders in the way of new and original observation. It is rather uncertain what microscopic instrument or instruments Malpighi used in his enquiries. He was four years older than Antony van Leeuwenhoek—whose portrait and life will be found in *ASCLEPIAD*, Vol. II., pp. 319—346, year 1885. They two were therefore cotemporaries almost of the same age; but Malpighi died, as we shall see, when he was sixty-seven years of age, whilst Leeuwenhoek lived until he was ninety-one, and in the latter half of his life did more microscopical work than in the first half. It is assumed by some, and I think fairly, that Malpighi made the first start in many of the discoveries, in respect to animal tissues, attributed to Leeuwenhoek, a fact that has a most natural aspect when we remember that he was from the first an anatomist and scholar, whilst Leeuwenhoek, a beadle in the Delft courthouse, was obliged to learn rudimentary anatomy as he made progress with his discoveries not only of structures observed through lenses but of lenses adapted and improved by himself.

As far as we know historically, Malpighi worked with a single microscopic lens not better than the half-crown lenses we meet with on the toy stalls of our day,

and possibly wanting in the stage and darkened pupil which these instruments possess. Certainly he had nothing so good as the microscopes which his less learned cotemporary ultimately produced and left to our Royal Society, to be unfortunately lost there.

The scientific labours of Malpighi and Leeuwenhoek, it may be mentioned, were almost equally acknowledged and assisted by our Royal Society, and their works appear most prominently in the Transactions of that society. The first notice we have of Malpighi in the Society's records relates to his "*Dissertatio Epistolica de Bombyce*," being a treatise on the natural history of silkworms. This treatise he dedicated to the Society in 1669, with the request that it might be published under their auspices. The request was complied with, and the work was put at once into the hands of the printer of the Society, and was published straightway with a licence signed by the President. This was during the time that Malpighi held the professorship of medicine at Messina. He had been elected F.R.S. the previous year—1668—and remained, the chronicles record, a constant correspondent until the end of his life.

To ascertain how true this statement is, I have taken the trouble to trace out the number of letters and treatises submitted by Malpighi to the Royal Society according to their dates. They run as follows. (1) A discourse on new discoveries in the egg (1671). (2) Three letters relating to the anatomy of plants and to his (Malpighi's) having seen "statutes" and pictures shining in the dark with flaming blue and white colours (1673). (3) A manuscript discourse on the anatomy of plants—with some elegant figures sent by sea from Venice (1674),—in which he suggested that the sun, by the force of its

heat, drives up the moisture from the ground into the roots of vegetables, and so causes the rising of sap, and that something like valves appear in the sap vessels of plants. (4) Observations and delineations of the structure of the galls of trees (1675). (5) A specimen demonstrating the circulation of the juices of plants (1678). (6) Notice, by Sir John Hoskyns, that Malpighi had observed that the reason of the mobility of the parts of insects after they are cut asunder is from the brain and medulla spinalis (1679). (7) A note that Laurence Bellini's medical works were printed at Bologna, in which was explained a course of physic by mechanical principles on a mathematical method, that Borelli's posthumous works were published, and that Father Bartoli, a Jesuit, had published a book on freezing and ice (1682). (8) A letter and discourse "(a) *De Cornuum Generatione*.—(b) *Uteri Fabricâ*" (1684).

The complete works of Malpighi, the "*Opera Omnia*," very copiously illustrated, were published by the Royal Society in 1686 in two volumes. The first volume contains the first part of the "*Anatomy of Plants*" and various letters to Oldenburg and Spon; the second volume contains the second part of the "*Anatomy of Plants*"; dissertations and letters treating on the silk-worm; the formation of the chick in the egg; exertations and letters concerning the brain, the tongue, the external organ of the skin, the omentum, adipose tissue and ducts; the anatomical structure of the viscera; and polypi of the heart and lungs. There is, in addition, a life of the author by himself.

"THE ANATOMY OF PLANTS."

The work of Malpighi which seems primarily to have attracted the attention of the learned is on the anatomy

of plants: "*Anatomes Plantarum Idea.*" It is richly illustrated, and forms a handsome folio volume, bearing date, in my edition, 1671. It was dedicated to our own Royal Society, and in producing it the microscope played a most important part. It was very natural that in the first examinations of structures by the microscope plants should form the special subject of investigation. The tissues of plants were at hand, and admitted of being mounted and examined with facility. Sections were cut with great delicacy, and their semi-transparency rendered comparatively easy the discovery of their structure. Beautiful sections are depicted, very similar to those which our young microscopists study from at the present day. It has been correctly said by more than one writer that Malpighi was amongst the first, if not actually the first, who detected sexual differences in plants, and showed in this way that plants were separated from minerals by a very wide distinction, and that they approached animals as things of life resolvable into classified families. He also discovered the vessels of plants, but, singularly, he does not seem to have detected that the sap or circulating fluid of the plant travelled through those vessels; yet he must have thought that some nutrient fluid traversed them, as he seems to have conceived the idea of valves as part of their structure.

It was natural that, in studying the natural formation of plants and of woody tissue, Malpighi should move from those structures to the structures of bone and teeth. He had observed that in plants the growth seemed in those which he had examined to proceed by the formation of rings or circular layers in the lateral direction. He conceived from this that bone was formed in a similar manner—"pari incremento procedit

natura in ossium augmento"—and although he was not altogether correct in his comparisons, there can be no dispute that he was one of the first, if not the first, to see the circular layers of bone and to indicate that bone has an organic tissue for its base. His weakness consisted in assuming a general from a particular growth, and this the more when he came to describe the structure of a tooth. In brief, he got but a glimpse of tissues, such a glimpse as led him, to a certain extent, into error, and yet, at the same time, caused him to open the way to our better understanding of the whole subject of animal construction.

THE "RETE MUCOSUM."

The anatomical structure of the body with which the name of Malpighi is most closely connected is the *rete mucosum*, or, as it is sometimes called, the *rete Malpighii*, in the layers of the skin. This structure, which, as we know, lies immediately under the epidermis, and which some anatomists consider to be the inferior layer of the epidermic tissue, was discovered originally and demonstrated by Malpighi. He considered this *rete* to be an independent structure, composed of fibres crossing each other, constituting a soft but distinct layer between the derma and the epidermis, and forming the bed or membrane in which the pigmentary matter of the skin finds its place. Much discussion has occurred since his day as to the accuracy of the original description. So great an authority as Bichat has actually doubted whether Malpighi saw a proper membrane, and has suggested that what was considered to be such a membrane was nothing more than a fine plexus or network of minute blood-vessels which passed through the *cutis vera* and deployed over its

surface. I think it is right to say that for a long interval the view started by Bichat in correction of Malpighi held its own. It was the view commonly received in my early days, but it has since been entirely set aside, to the advantage of the original observer, so that our modern students are taught definitely in the best of text-books that "the *rete Malpighii* is composed of several layers of epithelial cells, of which the deepest layer is elongated in figure and placed perpendicularly on the surface of the corium, while the succeeding laminae consist of cells of a more rounded or polyhedral form, the contents of which, soft, opaque, granular, and soluble in acetic acid, are often marked on their surfaces with ridges and furrows, and are covered with numerous fibrils, which connect the surfaces of the cells."

Malpighi knew nothing about cells as we do, but his general idea of construction was quite astounding for the day in which he lived. All that Bichat has written in criticism of him has turned out incorrect, and it may safely be said that the foundations of truth were laid by his skill, and that as foundations they remain unassailable. It seems to me, indeed, that Bichat, illustrious as he was, one of the few immortals in medicine, one historically amongst the first rank of all our craft, in this criticism merely followed the lead of Ruysch in his description of vascular structures.

Malpighi was of opinion that from the *rete* the colour of the skin is derived; in the *rete* he supposed was retained the colouring principle (which we moderns would call pigment), and which distinguishes, by colour, the races of men. Blumenbach entirely corroborates him on this point. "The skin of most animals, and of men consists of three parts: the external epidermis, or cuticle; the *reticula mucosum*, called from its discoverer the

Malpighian; and lastly the inner or corium. The middle coat, which very much resembles the external, so that it is considered by some as another part of it, is evidently more spongy, thick, and black in Ethiopians; and in them, as in the rest of men, is the primary seat of the diversity of colour." For, he argues, "in all the *corium* is white, excepting where, here and there, it is slightly coloured by the adhering reticulum; but the epidermis seems to shade off into the same colour as the reticulum, yet being transparent as a plate of horn, it appears, even in black men, to be scarcely grey in colour, when it is actually separated." But in another part of his works, in treating of the skin, Blumenbach rather modifies his views. He tells us that the corium is lined with a very tender mucus, affording a sort of glutinous bond, by which the epidermis, stretching over and protecting the surface of the body, adheres to the corium. This reticulum, he continues, just like the epidermis, is a most simple structure, entirely destitute of nerves and vessels, and differing from the nature of the corium.

Taking all the facts into consideration, as well as the arguments, we may safely give to Malpighi all the credit of discovery of the cutaneous membrane which bears his name, and we may give to him a further credit of detecting that the external colour of the different races of men is not due to any difference of colour in the blood circulating through them.

RESEARCHES ON GLANDULAR STRUCTURES, ESPECIALLY
THE KIDNEYS.

From the date of the publication of his original work Malpighi has been accredited with being the first anatomist who described the structure of the conglo-

bate glands. As far back as 1784 it was claimed for him that, with regard to the liver, he discovered its texture by his microscope ; that he observed the organ to be made up in its substance of innumerable lobules, which are very often of a cubical figure, and consist of several little glands, like the stones of raisins, so that they look like bunches of grapes, and are each of them clothed with a distinct membrane. Also that the small branches of the cava porta, and of the biliary ducts, run through all, even the least, of these lobules, in an equal number ; and, that the branches of the porta act like arteries, which convey the blood to, and the branches of the cava are as veins which carry the blood from, the little grapestone-like glands. From whence it is plain that the liver is a glandulous body, with its proper excretory vessels, which carry away the bile, the constituents of which were in the blood.

In the main this exposition is correct, as is also, to some extent, another exposition from him respecting the texture of the spleen, which organ he describes as made of innumerable little cells like honeycombs, in which there are vast numbers of small glandules, resembling bunches of grapes, which hang upon the fibres, and, fed by twigs of arteries and nerves, send forth blood, there purged into the ramus spleneticus, which carries it into the liver,—rather a crude physiology of the spleen, but, compared with what had gone before, a remarkable advance. To this day we still speculate as to the nature of the Malpighian corpuscle of the spleen, whether it has or has not a capsule, and what is its precise function.

The great achievement of Malpighi, however, relates to his researches into the structure of the kidney. He seems to have come to the conclusion that in all glands

there is one basic structure, namely, a cavity or follicle, in which secretion takes place. If anything can be called a material discovery leading to a philosophical induction, this was one of the most striking and valuable, and although Ruysch, led away by his fascinating skill in the process of injection of blood-vessels, controverted the idea of an intermediate body destined to cause secretion, and argued that as the injecting artery terminated direct in the duct, therefore secretion must take place in the artery itself. Ruysch had, I believe, few followers, and happily never succeeded in overthrowing the master who preceded him.

I may say I think, without diverging in the least from historical accuracy, that up to the time of Malpighi no reasonable account had been given of the minute anatomy of the kidney. In order to arrive at the state of learning on this point in his day we have to turn to the text-book of anatomy then best known,—I mean the *Mikrokosmographia*, a description of the body of man, by Dr. Helkiah Crooke, published on the last day of May in 1615. Crooke was enormously indebted to Vesalius for his illustrations, but he was a learned man, and many of his chapters are rich in historical quotation from the ablest of his predecessors. In short, his book is physiological as well as anatomical, and must have been a kind of encyclopædia of the Faculty of the period. His description, therefore, of the kidney must be studied as preliminary to Malpighian philosophy.

Crooke described that the kidneys are two vital organs covered with a double membrane; one outward, arising from the peritoneum as if it were wrapped in it; the other very thin, and surrounding the organ, making the outside glib and shining. The substance of the kidney is a hard flesh, most like the heart, except that

it wants fibres. It has two venters, or cavities, one outside, which Fallopius called the gate, and one inside. It has also its artery, vein, and nerve, and a vessel emerging, called the ureter. In a series of tables or plates, taken chiefly from Vesalius, the kidney, both of the man and the dog, is shown laid open, and although the cortical and medullary structures belonging to a kidney are not described in the text, they are faintly shadowed in the drawings, and a series of "caruncles or teats" are shown, each one like a calyx half surrounding a vascular body. There is, in short, a rough outline of the true thing, and it seems quite to be understood that the secretion of urine is derived from the arterial blood to pass away by the ureter.

Thus much of the kidney according to Crooke. Malpighi takes up the parable with some sort of microscopic lens in his hand, and begins to learn the true structure. He sees a surrounding or cortical substance immediately beneath the capsule, a substance of red colour, of granular consistency, and very vascular. He observes that this, like a roof, or arch, extends over a series of bodies lying within itself, and which he calls the pyramids, the base of each pyramid being connected with the outside or cortical matter above, and the terminal point, or nipple, of each dipping into the internal cavity, which we now style the pelvis.

Malpighi also observed that the pyramids were composed of a series of minute tubes, not, however, called tubes until probably a later date, when Laurence Bellini gave them the name of uriniferous tubes. But he did more than this: he discovered the little red bodies, scattered so freely through the cortical structure, now called the Malpighian tufts. He knew that these were vascular in their nature, and he conceived that from

them the urine was secreted or derived, and was given over by them into the tubular structure of the pyramids, to be discharged by the ureters, and so to the kidneys. That he stopped here is no wonder, seeing how many of his successors went no further, and that it remained for one of our own generation—one whom I had the felicity of knowing, the late Sir William Bowman—to work out the problem of the capsule, the Malpighian corpuscle, the convoluted tubule, and the rest of the anatomical physiological problem, which every student in these days is bound to learn.

RESEARCHES ON THE CIRCULATION, AND ON BLOOD.

That a cotemporary of William Harvey should have turned his attention to studies relating to the circulation of the blood, and to the blood itself, is nothing more than would be expected. Harvey, great as was his work, left one point obscure; nay, not obscure alone, but in absolute darkness. He demonstrated clearly enough that the arteries poured their blood into the veins, but in what way they performed that feat he left altogether an enigma. He did not indeed undertake to speculate on this matter, and one might almost suspect that in the blaze of his astounding discovery, he considered such a detail as unworthy of his regard. Or perhaps he may have felt himself too old and too wearied to take up the study of this topic. Or perhaps again, and this may be still nearer the mark, he was personally unacquainted with the mode of investigation by that new eye of his day, the simple microscope. We know that with regard to Aselli and his discovery of the lacteal system, Harvey was touchy and apparently critical, showing something of the same petulancy as he had observed in other men relative to his own discovery.

Yet the detail was really necessary, for if, under the new eye, there had been found no passage between artery and vein in the structures of the body, the theory of the circulation of the blood might have been imperilled.

Malpighi himself made the discovery that was essential to complete the Harveian triumph.

In the article on Leeuwenhoek in ASCLEPIAD, Vol. II., p. 337, the question of the discovery, visibly, of the circulation from artery to vein was discussed at considerable length. It is there stated, p. 337, that Leeuwenhoek was not the actual discoverer of the capillary circulation, but that he gave the final and unmistakable proof of it through the sense of sight. He himself, indeed, made no claim, although the discovery is so generally attributed to him. The true discoverer was Malpighi.

The history of this original observation is rendered in Malpighi's second epistle, *De Pulmonibus*, an epistle addressed to Borelli, with whom, as I have already shown, he was in intimate communion. It bears date 1661, and as Leeuwenhoek's description was in 1696, Malpighi is thirty-five years earlier. The lung structure would present a tempting field for new microscopical observation, and Malpighi turned to it with great success. By means of his lens he discovered the passage of the blood through the membranous part of the lungs, after which he directed his attention to the mesentery, in which structure he noted the same phenomenon.

Another discovery of a microscopical kind connected with the lung structure was that of the bronchial surface and the course of the air tubes. It is but fair to give to Malpighi the credit of having traced the course of the bronchial passages to their minute, if not

to their minutest, disposition. He had not the means at command to display to him the vesicular termination of the air passages, but he traced the tubes very far towards their termination, and he pointed out most correctly the manner in which the air reaches the blood, so as to communicate to the blood its vital parts. By this exposition he threw a direct light upon the phenomenon of respiration, and defined what was the meaning of that process ; thus paving the way for the after-researches of Mayow, who originally suspected the absorption of a nitro-aerial spirit, since designated oxygen.

From the mechanism of the circulation, Malpighi passed to the blood itself, and again showed unusual correctness. Being in practice as a physician when blood-letting was a common remedy, he could not fail to note the appearance of that buffy coat which forms on blood in inflammatory cases of disease. He entered slightly into a description of this phenomenon, but his skill consisted chiefly in distinguishing that blood is made up of three parts : of the white matter which we call fibrine, of the red-coloured corpuscles, and of the watery or serous portion. He apparently knew that in healthy blood the crassamentum, or coagulated substance, is made up of fibrine, holding and mixed with it the red bodies, corpuscles ; and it is obvious he saw these corpuscles with his microscopical lens, although we may be pretty sure that he did not understand their structure, nor so fully appreciate their physiological value as we do in this day.

To the profession generally the labours of Malpighi above recited are those likely to be of greatest interest, but they by no means cover the ground over which he traversed. His articles on the formation of the chick

in the egg ; on, what may be called, the natural history of the silkworm ; his anatomical disquisitions—"De Lingua de Cerebro," "De Externo Tactus Organo ;" "De Omento Pinguine, and Adiposis Ductibus ;" and "De Viscerum Structura"—have become classical, and are blended in his hands with the works of Willis, Glisson, Fracassati, and Bellini, to all of whom he gives the warmest acknowledgments. His treatises on the liver and on the cortical structure of the brain, as well as those on the kidney and spleen, are truly classical, and, although out of date, deserve to be referred to as guides to the history of discovery on the subjects to which they relate. His essay on the liver, whether we regard the description of liver structure, the production of bile, the bile duct, or the nature and the uses of the bile, are more than curious, containing many original thoughts which must, to many of his colleagues, have seemed strange, if not incomprehensible.

MALPIGHI AS A PRACTITIONER.

By a kind of common consent the cotemporaries of Malpighi have considered that in practice he was not of much consequence. Their ideas on this matter must be accepted, however, with many considerable grains of salt. The same argument was used against William Harvey, but not without some qualification, for Harvey, it is certain, instructed the surgeons in their operations, used the speculum for observations on the uterus, wrote a volume, unfortunately lost, and acted as physician to a large hospital. Malpighi practised, we may presume, much in the same manner, and we have no reason to doubt his being a very good practitioner. All that can be said against that view, indeed, is based on evidence purely negative ; that is to say, he did not leave behind

him much that is called practical. The reasons why he did not are obvious.

In the first place, he would have little time for observing, recording, and publishing so-called practical details. He was by nature an experimental observer, an observer bent on original investigation, living apart from the current thought and practice that surrounded him. He was learning a new Science, and, while he honoured the past, was more wistful of the future than of the present. He would probably mix little with those who lived in his sphere, except with such men as Borelli and the very few of that stamp. The fact that he sent his best works to the Royal Society of England for discussion and publication is all-sufficient as showing that he had little scope for original display in his native country. The many of his cotemporaries, therefore, would have very few opportunities of knowing much of his ability as a practitioner, and, as he would be talking a sort of scientific mystery on topics peculiarly his own, they would be sure to extend his mysteriousness to those topics with which they considered themselves most familiar. He would be a clever man, a learned man, a peculiar man, a genius, in their estimation, and that would finish him as a practitioner. How could he know what they knew, when their ideas of bodily organs and functions were in one direction and his in another?

In the second place, his own mind would be very much out of tune with the current ideas of practice, a fact which is elucidated by a remark of his against too profuse blood-letting in cases of acute disease. He would see, as the range of his own vision enlarged, what a number of absurdities existed in practice, and held their place as if they were the most distinctive gospel. Not combative in his nature, and too occupied to be

very persuasive and patient in his teaching, he would not be a man who would in his own time and place make many converts, to his own peculiar explorations, nor would he, indeed, see the full value, or application, of his own discoveries. He would wonder and go on, not waiting for results of a startling kind, but feeling sure that good results would one day be the fruits of his exertions. This is the right tone for an original observer to acquire, and it is doubtful whether a truly original observer could work under other influences, because if he stopped at the *Cui bono?* he would most frequently have to stop altogether. Malpighi certainly did not rest at the *Cui bono?* and therefore would not be rated as a practitioner. What mattered it about the structure of the kidney to the practical men? They wanted a medicine which would act on the kidneys, a diuretic; or, a medicine which should not act on the kidneys, or a medicine that should stop over-action. That was practice. Malpighi's name, going down with a piece of microscopical anatomy called the Malpighi tuft, was history—very creditable to Malpighi, very honourable to Italy and Bologna, but it was not practical; therefore, practice, to Malpighi, would not be an exercise possessing many attractions. He would do his duty, leave the rest to nature,—and throw all pills, salves, and nostrums generally, aside as too often the scybala of medical digestion.

For these reasons, singly and combined, Malpighi would not rank amongst his medical cotemporaries as a remarkable practitioner. Probably, indeed almost certainly, he would not be what is denominated a popular practitioner. Yet, that he had his admirers is proved by the circumstance that the new Pope, who had known him at Bologna, no sooner assumed the

tiara than he sent for him to his court, installed him there as his physician, and loaded him with honours. I do not wish to convey from this that such recognition carried with it any proof of scientific distinction in his profession; for courts are, it may be said, poor judges of true merit, and are as likely to ruin as to make the reputation in history of a truly great man. I merely wish to indicate that some men of learning and position must have trusted him even from the bare practical point of view, and honoured him for his worth as a physician as well as for his skill as an original investigator and for his wisdom as a philosopher.

Lastly, on the practical side he was not lifeless. With true insight, he detected that the fibrous part of the blood and the plastic exudations of pleurisies were of the same nature, and that the white fibrous masses, called by his predecessors polypi, and which—as I have shown in my work on fibrinous deposition—were actually believed to be polypoid animals,—came from the blood directly, were separations or deposits from the blood, and were the mechanical causes of death in many lingering diseases.

FINALE.

I must rest at the end of his labours, when he rested from them, not full of days, for he was only sixty-seven years of age, but full of fame and honour. The choicest recognitions had been paid to him. The Academy of Arcadians in Rome had received him into their body. The Royal Society of London had not only enrolled him as one of themselves, but, by the publication of some of his choicest works, had given to him a most distinguished place in their archives. His

own University of Bologna had raised a monument to his memory; and, during his short residence in the Eternal City, a medal was cast, on which his own features were rendered on one side, and a design bearing upon his botanical and microscopical studies on the other. He was admired, and he was beloved, as one of gentle life and persistent industry. Physically, his brain was in him the most active organ, and suffered first. On July 25th, 1654, whilst in the Quirinal Palace he fell, under a stroke—what a telling word it is—of apoplexy. There was rupture of cerebral vessel with effusion of blood into the ventricles, effusion that continued to increase, for he did not at once succumb, but, paralysed on one side of his body, remained helpless until November 29th immediately following—four months and five days. Then, under increasing pressure from an unusually large mass of blood in the ventricles of the brain, he breathed his last.

Malpighi, according to the custom followed in his day on all great people, was embalmed, and his dead body, taken with great ceremony to Bologna, his native city, was entombed, with much pomp, in the church of St. Gregory, his final resting-place. A statue there was also erected to his memory; but his best title to lasting honours is in our own Royal Society in the boarded volume.

MARCELLI MALPIGHII.

PHILOSOPHI ET MEDICI BONONIENSIS.

E

REGIA SOCIETATE.

OPERA OMNIA.

ORIGINAL RESEARCH, EXPERIMENTAL
AND INDUCTIVE.

"Dire n'est rien ; faire est tout."—RENAN.

RESEARCHES ON AMYLENE.

CCASIONALLY in these pages references have been made to the physiological action of the volatile fluid known as amylene, on which I once conducted a careful series of researches. I proceed in this place to supply certain further and more complete details of that inquiry; they may be of use to some future observer, and may lead him to investigate further what was for one short period in the past a subject of peculiar interest.

HISTORY OF AMYLENE.

The substance called amylene was first separated by Balard, of Paris, in 1844. It attracted but little attention until 1856, when the late Dr. John Snow having read of it, and learned theoretically its composition and physical characteristics, came to the conclusion, on theoretical grounds, that it was possessed of anæsthetic properties. In process of time he obtained some of the fluid, found that his theory proved correct in practice, and was so satisfied with the action of the substance that he employed it as an anæsthetic in 238 cases. M. Giraldis, of Paris, also employed it in 79 cases. I used it myself in connection with Snow in three cases, and many other

physicians and surgeons used it also. For a time, indeed, it seemed that amylène would, in the end, take the place of chloroform, but the promised success was marred by the occurrence of two fatal accidents, from its inhalation, in the hands of Snow himself.

Since that period (July 30th, 1857, date of the second fatal case,) amylene fell into entire disuse. Snow during the last year of his useful life returned to chloroform, and I have occasion to know, from my intimate friendship with him, that the occurrence of the fatal accidents caused him as much anxiety as surprise. It has been suggested since Snow's death that the specimen of amylene which he used was not pure, was in fact a compound of various substances. This I think was incorrect. He had administered from the same supply successfully in many other cases, and I experimented with a little that remained from the same store, without producing any dangerous symptoms, or detecting any peculiarity of composition.

PRODUCTION AND PHYSICAL PROPERTIES.

Amylene is made by acting on pure fusel oil or amyl alcohol with chloride of zinc; it has also been obtained, together with hydruret of amyl, by the action of zinc on iodide of amyl. In the distillation of the amylene other products are almost certain to pass over, and I think I am right in saying that the substance has never been obtained *absolutely* pure for physiological purposes. It ought, when quite pure, to be neutral to potassium, and to preserve that element from oxidation; but, as far as I am aware, this degree of purity has not been secured. The liquid has an oily character, and an odour compared by most persons to raw whiskey. Its sp. gr. at 60° Fahr. is 0·689; its true boiling point 95°

Fahr.; it is soluble in alcohol and ether to any degree, but it is soluble in water to the extent only of one part in 9319; vapour density is 35.6. As a fluid it is nearly colourless, and its vapour burns in air. It is composed of two elements, C_5H_{11} . Water dissolves about 2.35 per cent. of its vapour, and tastes of amylene; blood would probably absorb about four per cent. more.

Antiseptic Properties.—Like the nitrite of amyl, amylene is antiseptic; and dead tissue placed securely in the pure vapour of amylene remains unchanged, but the great difficulty consists in retaining the vapour in the vessel. If 60 grains of amylene be placed in a jar having a capacity of 80 cubic inches, the organic substance to be preserved being previously put into the jar, sufficient amylene may be diffused by the warmth of the hand to enable the specimen to be kept in preservation. Animal tissues in amylene-vapour do not gelatinise as in ammonia; they become soft, evidently from the action of water retained in them, but not offensive. Decomposed animal tissue placed in vapour of amylene does not become less offensive, although the process of putrefaction is for a time arrested.

Amylene preserves blood from putrefaction so long as it is present through the mass. During some weeks of extreme heat I retained blood unchanged in my laboratory by simply treating it, from time to time, with sufficient amylene to render the odour of it detectable. After amylene had been for a time in contact with blood in a closed bottle, the odour evolved resembled rosemary so closely as not, by odour, to be distinguished from it.

Effects on Flowers.—Amylene prevents the decomposition of fresh flowers, but it is not applicable as a preservative, owing to the change it produces in colour.

The leaves pass from green to a dirty brown, the yellow colour grows slightly dingy, and the red fades. The colour least influenced is yellow.

PHYSIOLOGICAL EFFECTS ON LIVING ORGANISMS.

Amylene does not in any degree irritate either the skin or mucous membrane; it acts feebly when exhibited by the mouth or by injection under the skin. It is by inhalation only that we can bring out its therapeutical properties.

I administered amylene in vapour to dogs, cats, guinea-pigs, and men. The symptoms presented in all are, in the main, the same. There is not the terrific excitement of the circulation which is seen when the nitrite of amyl is used, but there is some excitement, and there is quick inebriation, followed by powerlessness of the limbs, sudden collapse, and coma, with total insensibility to pain, but not always, and indeed rarely, with an equivalent loss of consciousness. In one case I saw a person who was under amylene, and entirely senseless to a severe surgical operation, talking with considerable correctness on the topics of the day, seizing objects with precision, and showing scarcely a perceptible gradation during recovery from mental inebriation to perfect and sober consciousness. From a psychological point of view this fact is doubly interesting, for the expression of consciousness is but a semblance. The person on recovery does not remember the period when, to the bystander, he seemed as wide awake as one unaffected. To test this phenomenon I myself inhaled amylene, and, before inhaling it, made up my mind that I would try for sensibility by pinching my hand. I inhaled, sitting in an arm-chair,

10 grains of amylene from a jar holding 100 cubic inches of air. The air was thus charged with rather more than 13 per cent. of amylene-vapour—a small quantity, except it be inhaled backwards and forwards in the manner I then followed, when the action was quickly decisive. Soon after inhaling I forgot myself altogether, but four minutes from the time I took up the jar I was quite conscious again, waking as if with a start. “Ah!” I thought, “this experiment will not do; the amylene acts so quickly there is no interval during which a man may test his own sensibility;” but turning now to my wrist, I found I was wrong in this suspicion, for there were deep marks of pinches in several places; and the bottle of amylene-vapour from which I inhaled had been replaced on the table with the stopper firmly adjusted, as I had intended. Thus I had, in fact, been performing acts of consciousness, preconceived and carefully carried out, without remembering any single fact connected with the action.

I have seen phenomena nearly similar under chloroform, but never so marked as under amylene. Snow and I had an experience of a striking kind. During a severe operation on a child under the influence of amylene, we saw the child playing with a ball, throwing it into the air, catching it with precision, and talking and laughing the while. The late Sir William Ferguson was the operator, and the operation was amputation of the foot.

These phenomena are remarkable as showing that the human mind, through its manifestations, may exhibit a mere objective consciousness apart from the ordinary subjective consciousness of daily life; may exhibit, *i.e.* a consciousness of which it is itself unconscious, and this under the mere influence of a volatile liquid which

mixes so indifferently with blood at 98° Fahr. that one part of it only will combine with a little over 10,000 parts of blood.

Analogous phenomena apart from experiment, and spontaneous or natural, are seen in somnambulism. The somnambulist is in precisely the same state as the experimentalist under this amyl-compound; he pursues acts of consciousness of which he is not self-conscious; he presents to us, *i.e.*, mere objective consciousness. Have we not here a key to the mystery hitherto so perplexing of somnambulism? I will not say it is certain, but the evidence is as clear as inferential evidence can be, that persons who are subjects of somnambulist movements do, through some abnormal process of digestion or respiration of the starchy elements of food, produce in their own organisms, by their own organic chemistry, an agent which, like amylene, destroys remembrance, and perhaps judgment and reason, but leaves the brain still able to act and to direct the limbs to do things which they could not do better in wakeful hours.

I notice this amylene dreaming, in which, during the period of insensibility, the mind is capable of carrying on certain proceedings upon which it was predetermined, because we have a similar phenomenon in our daily experience, in the fact that we can, under pressure, go to sleep determining that we will wake at a certain time, and do so almost to the minute. In both these cases it seems as though the dark sleep were pierced by one pervading conscious gleam which obeys the orders it has received, and watching the slumbering spirits around it, calls them forth into activity at the proper time.

The insensibility produced by amylene is very com-

plete but exceedingly transient; recovery after the vapour is withdrawn is sudden, and it is therefore necessary to keep up the effect by constant inhalation of large quantities of the vapour. Between the period of full sensibility and complete insensibility there are three well-marked stages. The first is one of mild excitement, during which the face becomes red and injected; then a period of staggering inebriation; and thirdly, a period of collapse and insensibility. Snow added a fourth stage of entire muscular relaxation, but I have myself not been able to detect it. During narcotism from amylene there is no convulsion, but not unfrequently there is a peculiar tremor of the muscles, very general, but, if I may use the term, minute. Carried to an extreme, the vapour kills animals. The only structural peculiarity found after death is engorgement of the right side of the heart. The lungs are healthy, and the blood is unchanged in its general physical properties. I would dwell on this last fact with emphasis in order to correct an error made by a reporter on amylene to the Academy of Medicine of Paris, to the effect that amylene removes the red colour from arterial blood, whereas chloroform does not. This is a mistake, founded obviously on an inference derived from imperfect observation. When amylene is added in quantity to arterial blood drawn from the body, it does unquestionably produce darkness of colour; but in the living animal, during its exhibition by inhalation, no such event occurs at ordinary temperatures; for I have taken the blood from artery and vein, and have compared the specimens. The colour of both bloods is normal, the period of coagulation is natural, and the corpuscles are unchanged.

Owing to the fact that amylene is feebly soluble in

water, and therefore feebly soluble in serum of blood, a very large percentage of it requires to be inhaled before any effect is produced. Snow estimated that the air must be charged with 40 per cent. in order to produce entire insensibility, and in fact he underestimated the quantity by working with water at a temperature of 56° instead of 98° Fahr. I find that, instead of 40, 50 per cent. of the vapour actually is required at blood-heat. But as the blood at its ordinary temperature can only absorb $\frac{1}{98,100}$ th part of amylene, it cannot, even in an adult man, receive at any time much more than from $4\frac{1}{2}$ to 5 grains. It is clear, too, from the rapidity with which recovery takes place, that the fluids of the tissues get but feeble impregnation.

In order to institute a physiological comparison, I tried the effect of the analogue of amylene, olefiant gas. Olefiant gas is the hydruret of acetylene, but, like amylene, it is a pure hydrocarbon; its composition is different in one respect, viz., that it is composed of two equivalents of carbon and four of hydrogen. As I had anticipated, the action was nearly the same; to produce perfect insensibility, 50 per cent. of the gas was required in inhalation. The effects also were transient.

The effect of amylene on the muscular tissue was studied in various ways. As will already have been gathered, amylene reduces the muscular power, but the effect is not abiding. In fact amylene approaches the class of negative narcotics, and enters into no chemical combination with the tissues. This fact led Dr. Snow to say that in action it almost resembled nitrogen.

When an animal has died from amylene, the peculiar odour of the substance may be detected in the tissues for many days. This circumstance may seem at first

sight to be opposed to the statements already made respecting the feeble absorption of the vapour by the blood. However, there is no contradiction; the phenomenon is due to the circumstance that amylene possesses the power of charging water with its own odour and taste when in extreme division. This fact was observed by Snow with his usual precision. "Amylene," he says, "requires 9,319 parts of water for its solution;" and, he adds, "the water which has dissolved this small quantity of amylene tastes as distinctly of it as amylene itself." I notice this peculiarity, as it might happen some day to be of service in medico-legal investigations.

The reason why the vapour of amylene is so fatal is, I believe, easily explained. It is too insoluble in the blood. In the blood column it separates in the form of vapour, and so acts like air introduced into a vein. I found in every case of death following its inhalation in the lower warm-blooded animals, sufficient evidence of the cause of death on this explanation. There were always bubbles of vapour in the blood, of the *venæ cavæ* and of the right auricle, even though the blood had cooled. On collecting the blood in a test-tube and applying the heat of the warm hand, more vapour also escaped, causing more bubbles and slight frothing of the fluid.

Owing to the low boiling-point of amylene I have used it as spray for the production of local anæsthesia by cold. No substance answers better for this purpose; it is more rapid than anhydrous ether, and more stable than any hydride, which in many respects it closely resembles in physiological action.

HYDRAMYLE.

AT the meeting of the British Association at Exeter in 1869 I placed before the Section of Biology a fluid called hydride of amyl. The fluid had a specific gravity of $\cdot 625$, and it boiled at 30° C. (86° F.). Its composition was $C_5H_{11}H$. Its vapour was a quickly-acting anæsthetic.

During the following year I again experimented largely with this hydride, with the view of rendering it applicable for the production of rapid anæsthetic sleep, but found it too light to be readily manageable; that is to say, it escaped from the inhaler, as a gas, by the mere warmth of the breath. The vapour had also an objectionable odour.

I set to work to obviate these difficulties, first by slightly weighting the fluid, and secondly by inventing an inhaler that should more effectually restrain the liquid as it was undergoing evaporation.

In making bichloride of methylene we put finely-pulverised zinc into a retort and pour upon it absolute alcohol and chloroform, using afterwards a heat not exceeding 120° F., in order to distil over the product. I modified this process by diluting the mixture of chloroform and alcohol with eight times the volume of hydride of amyl. This mixture was poured upon the zinc, with the result of an instant strong action without any application of heat. After a free evolution of gas, which lasted some minutes, there distilled by this method a compound fluid which contained pure hydride of amyl and pure bichloride of methylene. If the distillation were carried on at 98° F., the fluid that came over had the specific gravity of ordinary ether ($\cdot 720$),

a most agreeable odour, and rapid anæsthetic action. I administered this fluid, in vapour, forty-six times for short operations on the human subject, and in the average of cases produced the required insensibility within fifty seconds. In one instance insensibility was produced, a firm tooth was extracted, with perfect recovery in forty seconds. No vomiting nor other untoward symptom attended the administration.

There was, however, a peculiarity in the action of this vapour to which I ought carefully to refer, viz., that insensibility from it intensified after the inhalation of it was withdrawn. Thus in administering, whenever there was the least indication of its effects, such as winking of the eyelids or drop of the hand, the sign was given to stop the administration. The operator waited a few seconds and then proceeded. The inhaler, constructed for the administration of this new anæsthetic, was a simple hollow cone made of leather, and furnished with two light silken valves for entrance of air and exit of vapour and breath. It was lined with domette set on a light frame or ring of metal. When the inhaler was not in use it formed a case for holding safely, in a bottle, four fluid-ounces of the anæsthetic liquid. This quantity was sufficient for twenty operations, of from one and a half to three minutes' duration, two drachms being the amount necessary for an operation not exceeding three minutes' duration. For long operations I did not expect it to replace the heavier anæsthetics. I was indebted to the late Mr. Ernest Chapman for the suggestion of the word Hydramyle for this compound.

COTEMPORARY PRACTICE AND LITERATURE.

"Every physician will and ought to make observations from his own experience; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together."—FREIND.

CONTAGION AND PHTHISIS PULMONALIS.

AS if to discount good influences of a true hygienic character in reducing the death-rate from phthisis pulmonalis, some enthusiasts, in writing on the contagion speculation, have endeavoured to show that the lessened mortality from the disease, observable during the past few years, is due to the influence of the doctrine of contagion. Dr. Lawrence Flick, of Philadelphia, has written in this strain in the *Medical News* for May 14th, 1892. He calculates that the actual decrease of deaths from phthisis in Philadelphia, from 1881 to 1891, is 0.734 per thousand of living people, which, with the present population, shows a saving of 784 lives a year. The result he assumes to be due to the circumstance that members of the medical profession in Philadelphia, having become converted to the hypothesis that phthisis pulmonalis is spread by contagion from particles of dried sputum derived from patients affected with the disease, have taken care that such contagious matter shall be at once destroyed, and so have prevented the spread of the disease by communication. It is astounding how speculations of the kind named are supported

on coincidences. There is a physician living in this country who, with equal argument, believes that the reduced mortality from the disease in question is due to a medicinal cure for the disease which he himself has introduced. To prove the beliefs of such advocates, it would have to be demonstrated how many practitioners have prescribed the means of prevention or cure specified, and how many times their suggestions have been practically acted upon. Impossible demonstrations! As to the first—the conversion of the profession to the hypothesis of contagion and the influence of that conversion in modifying the death-rate—it is the simplest truth to say that if the profession in Philadelphia is at all like the profession here, there is not a shadow of evidence in support of the proposition. There are a few probably who accepting the hypothesis out and out are more particular respecting cleanliness and removal of expectoration than they were; whilst the many, looking at the hypothesis doubtfully, are as little influenced by it now as they ever were. Why should this not be so? The experience gathered in Naples on this subject, when the most rigid rules for destruction of all morbid material failed to render service, is not likely to be forgotten; while the majority of observers have never been able to see the evidence that would establish the hypothesis. Let me take in illustration my own experience. In my student life I was under an able teacher of the practice of medicine, the late Dr. Andrew Anderson, of Glasgow. In his lectures on phthisis, he entered carefully into the contagion theory, at that date—1846—rather popular. He held a wise reserve on his own part; but he told us the question was a vexed one, and he urged us all

to watch most carefully for ourselves in our future lives. I was one who determined to watch intently, without bias—how could I have any occasion for bias?—and persistently. I have sustained my watchfulness in the same spirit from then until this day, *i.e.*, over a period of nearly fifty years. Every opportunity has been afforded me for watching. I entered early in life into the experience of a large general practice; I left that for physician's practice in London. I became physician to an infirmary for diseases of the chest, situated in a densely populated part of London. I held my post for about fourteen years, rising to the senior physicianship. During a part of this same period I acted as one of the physicians to the Metropolitan Dispensary, the Blenheim Street Dispensary, and the Margaret Street Dispensary another institution for the treatment of consumption. This was a wide field for practice and for observation. Moreover, at these Institutions, where I gave systematic instruction, I was fortunate in having able assistants, who kept careful notes of the cases under treatment. In the course of the period named, no fewer than three thousand patients suffering from phthisis came before me; and during all the time, the question of evidence on the point of contagion was never for a moment lost sight of. It is the fact that in all this field of practice not a single particle of evidence ever came before me that sustained the contagion theory, or gave, on reliable data, the least colour to it. No professional brother, no assistant, no nurse, no person connected in any way with the phthisical sick, ever afforded indication of having received the disease by communication; and it is worthy of remark that no physician of my acquaintance, in the days I speak of, ever expressed a suspicion of communication

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by contagion. In the fifties, we had in London a society for the special study of chest diseases. In it were men of acute observation, and of skill in diagnosis. We were all engaged in hospital practice, and we all worked earnestly. The late Drs. Baly, Snow, Leared, Sibson, Francis Webb, Risdon Bennett, Edward Smith, were members of this society, as were Sir Richard Quain, Dr. Hawksley, Dr. Cockle, and one or two more whose names escape me. We met in turn at each other's homes; we drafted patients to our meetings for careful study; we discussed every point with the utmost freedom of discussion; but I feel sure at that time not one of us entertained seriously the hypothesis of contagion. Was it that we were all blind to evidence which must have been passing every day before our eyes, evidence which should have occurred to us, apart from any hypothesis, if it existed? Was this revived hypothesis wanted to open our eyes? Or was it that when the hypothesis was once re-started those who accepted it did not really see what they had failed to see previously, but were led to adapt their discernment to the requirement and to receive what was necessary to be received in order that the hypothesis might stand on a logical foundation?

I resigned my work as physician to the special institution for diseases of the chest towards the latter part of 1867, but I have remained up to the present time profiting from another course of experience in a practice of twenty-five years. In the latter part of that time much controversy has been revived on this subject of the communicability of phthisis by contagion, and although I have taken little public part in the controversy, I have been as active in observation as ever I was, endeavouring to learn, without th

prejudice, the exact facts. For many years I have sat at a board of insurance directors, feeling all the responsibility that attaches to that official duty. In every case of phthisis pulmonalis that has come under my notice in practice, and in every case of the same disease of which I have had cognisance otherwise, I have been vigilant to arrive at the circumstances under which the first symptoms of the disease were manifested. Not once has there been evidence of origin of phthisis from contagion on which I could rely. It is true I may more than once have heard the conclusion that some contagious influence was at work, but on further inquiry the evidence has broken down. There were living near to me two sufferers from the malady who occupied the same house. They were brother and sister, the only children of a professional friend ; they had the hereditary history painfully characteristic of phthisis, and in the course of the affection in them from the beginning to the fatal end their symptoms ran so closely as to be like twin phenomena. Could there have been contagion in these cases ? Many might have said so who had not taken pains to investigate the facts. To me the facts were familiar. The brother was often assisting me in my work, so that I knew his history ; and his attack dated from a severe cold contracted by exposure to an intense chill at an evening party, to which he went lightly clad, and in which he added to the risk from changing portions of his dress for some much lighter ones in order to take part in a charade or play. He sent for me at once when he became ill. It was the old story of pneumonia followed by rapid tubercular phthisis. The sister at this time was many miles in the country, where she had been for several months, during which time the two had not once met

each other. She, like her brother, took a severe cold; and when she came home, and passed under my care, she was in the same state of phthisis as he. In neither case was there the remotest evidence of contagion, yet because of their affinity I afterwards heard it adduced that contagion was at work. Not infrequently on being called upon to prescribe for phthisical patients the statement has in late times been made to me that the disease had been contracted by contagion, but in not one instance has any proof been supplied that would in a court of law of the first instance bear investigation.

I ask naturally in conclusion, Can I be blind to open facts, or do I see correctly, and has the hypothesis of contagion blinded those who see differently from myself? Willingly I will admit myself blind, and will submit myself to any correction that will give me sight; but the correction must be supplied, and in a form that makes all so clear that he who runs may read. I must be shown one case of phthisis in which the evidence is absolute that the disease was contracted from contagion, and that it could not possibly have been produced by any other determining cause.

Returning to Dr. Flick and his argument—that the mortality from consumption in Philadelphia, or any other city for the matter of that, is reduced by the precautions taken in regard to the disposal of the sputum of afflicted persons—the point is clear enough that what he traces as cause and effect is due to nothing more than coincidence.

No sanitarian for the past forty years, to my knowledge, has doubted the fact that phthisis pulmonalis will disappear just in proportion as broad sanitary views are established and brought into pr We have

all believed, and, as we think, proved, that under new conditions, in which there is provision for a better heredity, in which cleanliness becomes the watchword of sanitation, in which the youth of both sexes can enjoy free exercise and recreation in the open air, and in which the labour of life in confined places is being reduced to a proper standard; we have all believed that with these changes the disease would die out altogether. In my *Essay on the Hygienic Treatment of Pulmonary Consumption*, published in 1856, these principles were so strenuously enforced that I was looked upon in enforcing them as a visionary, and as wanting in that practical acumen which was necessary for successful effort in the treatment and prevention of disease. I was not alone in that argument, but I was one of a very few. In time our principles prevailed, and the reduction of mortality from consumption in military barracks, owing to reforms which were insisted upon in our teaching, afforded direct evidence, long before this contagious idea was advanced, of the truth and correctness of our observation.

The process of purification has since then gone on by leaps and bounds, and we are beginning to see the good results; a new hypothesis springs up coincidentally which may or may not be right to a certain extent within its own narrow range, but which has no more to do with the great results at this time boasted of than the reduction of typhus in its worst forms has to do with carrying of camphor in the pocket, or placing rue in the wardrobe or on the desk before the judge of assize. It may be quite practical, it is quite practical, to take care that the expectorated secretion of consumptive patients is destroyed. The late Dr. William Farr put his finger on this fact, and every sanitarian felt the importance of

his advice. But this is merely a matter of cleanliness, merely a part of the whole system of perfect hygiene; and if we let this one thing stand as if it were the whole thing, we shall merely lead to carelessness in regard to everything else, and shall make matters worse instead of better. Dr. Flick is so good an observer and so earnest a man, there can be no hesitation in being critical on this particular part of his instructions, and it is only because it is painful to see him narrowing himself to one issue that this criticism is ventured upon. I hope he will take a wider view as time rolls on, and accepting the old message of the Mishna, that "Outward cleanliness is inward godliness," or, as John Wesley transformed it, "Cleanliness is next to godliness," will become a preacher of true catholic doctrine in all that relates to sanitation and the physical welfare of the world.

"CHINESE CENTRAL ASIA."

TWO new volumes of travel by the Rev. Henry Lansdell, D.D., (Sampson Low & Co.) form an important addition to the literature of this century; the more important because the work is not a compilation, but is a true history of the original observations of the man who wrote it, and who transferred to paper many of the important scenes he witnessed while they were still in his sight. The work of Dr. Lansdell on Russian Central Asia bore evidence of the courage, endurance, and research of its author. The present work is a companion and complement to its predecessor. In it are described the journeyings through Chinese Central Asia, by which is meant that portion of the Celestial Empire lying outside the Great Wall, that

is to say, external or extra-mural China. The main object of the journey the author himself tells us was to spy out the land for missionary purposes, and although he himself did little missionary work, because as a preacher he was dumb, and as a distributor of literature he had not the proper translations, he acted as a pioneer, considering it his task to observe what openings existed or could be made for others to follow.

Readers who like the study of books of travel will find in these volumes page upon page of matter which is sure to be to them a kind of travel of their own, through a *terra incognita*. Here, too, they will discover little bits of description of medical value.

In the second volume, beginning at page 321, there is an account of a ride on the road crossing the Karakoram Pass which is very peculiar. The traveller here reached an altitude of 18,550 feet, the whole region bleak, dismal, inhospitable, without fuel or forage; the weather, however, being fine and calm, he and his friends enjoyed the thought of mounting 3,000 feet higher than Mont Blanc, and riding over, perhaps, the highest pass in the world. Fortunately there was no snow on the pass, and the effects of the altitude on the bodies of the travellers were less painful than they expected. Lansdell says that his breathing was not much distressed so long as he remained in the saddle, but a very little walking sufficed to induce distress.

In another part of the same volume, page 269, where he had to climb a long steep gradient through snow more than a foot deep, he reached a small platform where he obtained a splendid view of the surrounding snow mountains, as well as of the saddle

above over which he had to climb. The ascent was very trying, but the sun shone, the air was clear, the ride enjoyable, and he and his companions reached the height of 17,000 feet. Then most of them, horses as well as men, were suffering more or less from *dam*, or mountain sickness, brought on by the rarefaction of the atmosphere. At what height he began to feel the new sensations Lansdell is not quite sure, but several times he dismounted to warm his feet by walking before he attained the great height, and so soon grew tired and out of breath he imagined his walking powers had become demoralised by being so little of late exercised. As he approached the snow-line he had a slight headache all day (very unusual with him), and it continued through the night. But it was not until he began to ascend the pass that he realised what the *dam* really meant, for upon being told that there were some partridges just over a hill a hundred yards off, he took his gun, slid off his yak (the beast that bore him), and began running up as on ordinary occasions. Before he had proceeded many yards, however, his heart began to beat as if it would burst, and he was obliged to sit down to take breath, and learn that such agility at such altitudes was out of place. In page 323, he tells us that he exchanged with the natives, for food, chlorate of potash, a supposed antidote for mountain sickness.

In an appendix there is a good list of the fauna of Chinese Central Asia; and taking them altogether, I do not think that any volumes of personal history on travel could be found conveying more of knowledge, of natural and general history, and of adventurous research

"BURDETT'S HOSPITAL AND CHARITIES ANNUAL, 1894."

THE fifth volume of this most interesting and important work has just appeared (The Scientific Press, 428, Strand, and Scribner & Co., New York). It places in the most handy and convenient form the facts relating to hospital and charitable work in the year 1893. It claims to be a year-book of philanthropy, and no one can dispute its character in this respect. Mr. Henry C. Burdett, to whom we are indebted for this compilation and commentary, has spared no pains to collect every fact that is worthy of note, and to arrange the whole under easy references. The volume is one that will in time be considered historical, and as it is constructed,—strange as it may seem, considering the thousands of details described,—it is really a volume easily read, and, to those interested in the work on which it treats, absorbing. Mr. Burdett is not only a careful collector of facts, but is impartial in his array of them, and outspoken in regard to the lessons which the facts convey. Thus his work enables us to grasp the great philanthropic efforts now being conducted throughout the kingdom for the amelioration of human diseases and miseries, as if we were being moved over the whole area, and making observations for ourselves. Of course in a volume of this nature there is much that is commonplace and merely descriptive, but the commonplace is foundational, and from it the most useful inferences are constantly being drawn, affecting the whole question of Charity Organisation. One inference of peculiar value is that the work of organisation is most irregular in mode of demonstration, in cost of demonstration, and in results. The

grand object of the author is clearly to show what could be done by uniformity of action, what economies could be realised, what wider fields of work could be opened without any increase of expenditure, and what numbers of persons could be treated, beyond what are now treated, with facility to the governors of institutions and benefit to those for whom they minister, if better order and system prevailed. That the Annual is increasing in popularity is obvious from all the indications connected with it. It has become a necessity, like *Hazell's Annual*, *Whitaker's Almanack*, the *Medical Directory*, and other similar works of reference. In a very few years it will become one with the people.

MEDICAL SPELLING.

DR. GEORGE M. GOULD, of Philadelphia, in a pamphlet reprinted from the *Medical News* of June 17th, 1893, challenges the method of spelling some medical words. He urges that the vast majority of medical words have not grown out of the old languages, either ancient living Greek or the mediæval preserved Greek. When a word is desired the modern minter snaps down his Liddell and Scott, finds some words that best suit his purpose, and shakes them together in his etymological basket until they cohere into some sort of unity,—not infrequently a very ludicrous combination. A little further he says that language was made for man, and not man for language. He commends strongly the plan of the American Association for the Advancement of Science in regard to all chemical words, and everywhere he suggests the leaving out of all superficial letters in every word; he thinks also that it is the duty

of representatives of the Healing Art to assist in the reform he advocates, however timid may be their advances. Dr. Gould, in this respect, sets an example which formed the subject of an article in the old *Journal of Public Health* as far back as the year 1856. The only fault is that he is too timid in what he suggests. It is not merely the making of bastard new words out of Latin and Greek that deserves reprobation; it is the habit of introducing at every turn some new word which shall seem to convey some old knowledge under a new name, an industry of idleness and pretence. Such terms as thrombosis, embolism, stenosis, and hundreds similar, with which the pages of our medical literature are now blotted, are making us laughing stocks of literature against whom even pedants have their joke.

TO CORRESPONDENTS.

In order to prevent the frequent and troublesome mistake of addressing communications to the "Editor," it is necessary to repeat that everything published in the ASCLEPIAD is exclusively by the Author whose name is on the title-page.

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Advertisements, and all Communications respecting them, should be forwarded to Mr. A. P. Watt, 2, Paternoster Square, London, E.C.

The Author will be greatly obliged if senders of Newspapers and Magazines would kindly mark the passages to which they wish to direct his attention.

4th
Quarter,



1893-4.

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ERRATUM.

I regret that in the last number, in the Review of DR. POWELL'S Book, Messrs. Longmans were, in error, named as the Publishers, instead of Mr. H. K. Lewis, 186, Gower Street, W.C.

EXCHANGES.

"The New York Medical Journal." "Journal of Chemical Society." "Journal of Society of Arts." "Chicago Med. Journal and Examiner." "New Orleans Med. and Surg. Journal." "Sacramento Med. Times." "Illustrated Science Monthly." "New England Med. Monthly." "Chemical News." "College and Clinical Record." "Glasgow Med. Journal." "Bristol Med. Chir. Journal." "Australasian Med. Gazette." "Journal of Mental Science." "Quarterly Journal of Inebriety." "Sanitary Journal." "Hygiene Pratique." "Med. Temperance Journal." "The Polyclinic." "Asylum Journal." "Journal of Microscopy." "Longmans' Magazine." "Gentleman's Magazine." "Belgravia." "Chemist and Druggist." "Health." "Sanitary World." "Journal d'Hygiène." "Malthusian." "Animal World." "Scientific American." "Med. Record." "Bulletin de l'Académie de Médecine." "Liverpool Med. Chir. Journal." "Medico-legal Journal." "Hospital Gazette." "Journal Scottish Meteorological Society." "International Journal of the Med. Sciences." "Journal of Heredity." "The Health Journal." "Science Gossip." "The Med. World." "The Photographic News." "Edinburgh Med. Journal." "Review of Reviews." "South African Med. Journal." "Phrenological Magazine." "Med. and Professional Review." "Vegetarian Messenger." "The Analyst." "The China Med. Missionary Journal." Shanghai. "Séances et Travaux de l'Académie des Sciences." No. 5. Par JULES SIMON. Paris: 1887. "The St. Louis Med. and Surg. Journal." "The Journal." "The Canada Health Journal." "The School Board Chronicle." "Our Day." "The Scientific Inquirer." "The Monist." "Public Health." "The Satellite." "The Band of Hope Journal." "University Med. Magazine." "The Journal of Physiology." "New York Abstract." "The Humanitarian." "Journal of the American Med. Association." "The Medical World." "Provincial Med. Journal." "Albany Med. Annals." "Diet and Hygiene." "The Open Court." "Journal of Physiology." "Journal of Nervous and Mental Disease." "Pictorial World." "Horticultural Times." "Med. Analectic and Epitome." "Journal of Dermatology." "The Med. Record." "Good Health." "Med. News." "Journal of British Dental Association." "American Journal of Med. Sciences." "Drug Topics." "Abkari, Indian Temperance Journal." "Woman." "The Central Review." "Rivista Internazionale d'Igiene." "Australian Med. Gazette." "Boy's Own Paper." "The Friend of China." "Provincial Med. Times." "The Canine Journal." "The Tocsin." "The Hospital." "The Gentlewoman." "The Author." "The Medical Magazine." "The Corpuscle." "The Dublin Journal of Medical Science." "Journal de Médecine de Paris." "The Medical Pioneer." "The Sheffield Medical Journal." "The Humanitarian." "The Indian Medico-surgical Review." Hutchinson's "Archives of Surgery."

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